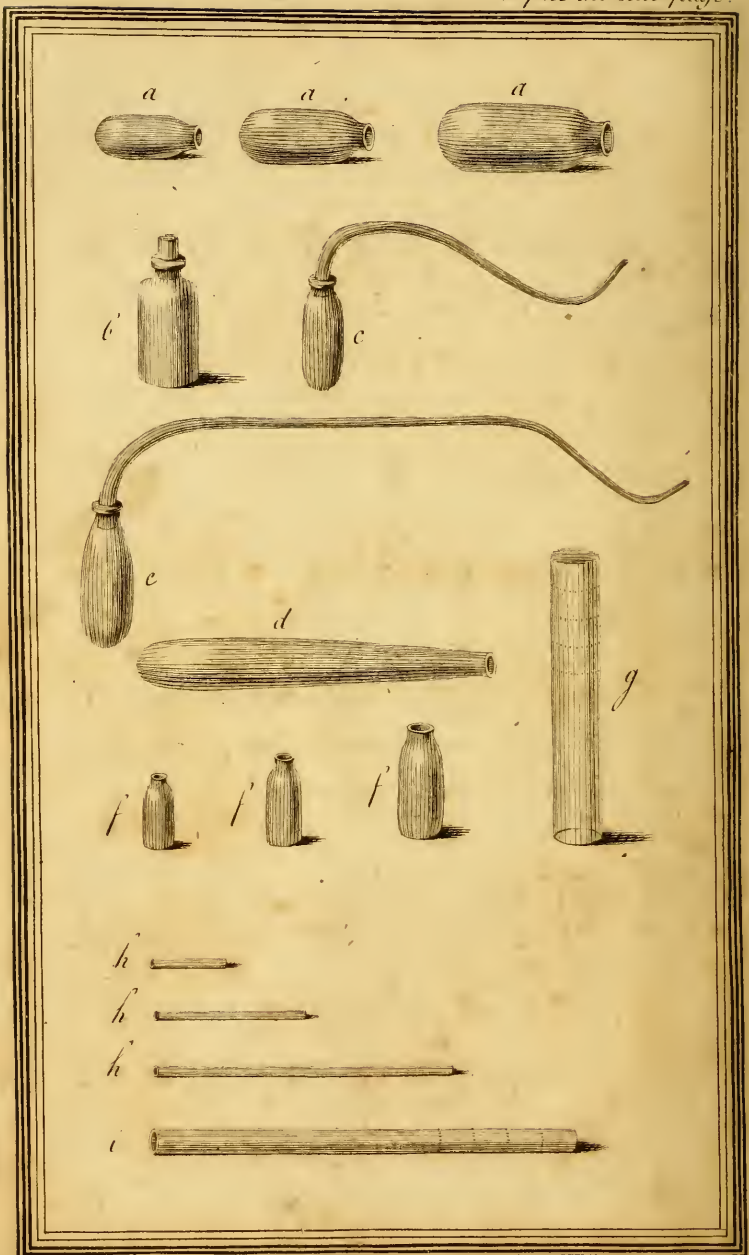


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EXPERIMENTS
AND
OBSERVATIONS
ON DIFFERENT KINDS OF
AIR.

VOL. II.

By JOSEPH PRIESTLEY, LL. D. F. R. S.

Ita res accendunt lumina rebus.

LUCRETIVS.

THE SECOND EDITION.

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T O

Sir JOHN PRINGLE, Bart.

PRESIDENT OF THE ROYAL SOCIETY,

THIS SECOND VOLUME OF

EXPERIMENTS AND OBSERVATIONS,

ON A SUBJECT TO WHICH HE HAS GIVEN
PARTICULAR ATTENTION,

AND IN THE INVESTIGATION OF WHICH HE
HAS PUBLICLY AND PRIVATELY EN-
COURAGED THE AUTHOR,

I S,

WITH THE GREATEST RESPECT, INSCRIBED,

BY HIS OBLIGED,

HUMBLE SERVANT,

London, Nov. 1775.

J. PRIESTLEY.

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T H E
P R E F A C E.

I HAVE seen abundant reason, since the publication of my former volume of *Observations on different kinds of air*, to applaud myself for the little delay I made in putting it to the press ; the consequence having been that, instead of the experiments being prosecuted by myself only, or a few others, the subject has now gained almost universal attention among philosophers, in every part of Europe. In consequence of this, considerable discoveries have been made by people of distant nations ; and this branch of science, of which nothing, in a manner, was known till very lately indeed, now bids fair to be farther advanced than any other in the whole compass of natural philosophy. The attention which my former volume excited, has been a motive with me to continue my

own researches, till I have been led to discoveries of more importance than any that I had made before, and of which I had not at that time the most distant idea. It has, likewise, been the means of extending my acquaintance among philosophers, of whose lights I have availed myself, as my narrative will witness.

Sig. Felice Fontana of Florence, Sig. Landriani of Milan, and Mr. Lavoisier of Paris, have already announced, in their late publications on this subject, that they have much more to follow with respect to it, and that they are at present intent upon the investigation of it. Mr. Montigny (when I had the pleasure of meeting him at Mr. Trudaine's, as mentioned in the course of the work) gave me an account of some very curious experiments which he had made on inflammable air, and which I expect he will soon communicate to the public. That veteran in philosophy, Father Beccaria of Turin, has also made some valuable experiments of this kind, and will, I doubt not, prosecute them with his usual address and success. Mr. Bergman of Upsal, who,

who, as I have said, wrote to me formerly on this subject, has since published a paper on fixed air in the Swedish language, which I cannot read. Several ingenious persons, whose names are not yet known to the public, are, to my knowledge, engaged in these pursuits; and we are not without expectations from the oldest *living fathers* of this philosophy, Dr. Brownrigg and Dr. Black, as well as other gentlemen in Scotland. Besides these, there must be, I doubt not, at least twice as many persons at work upon this subject, as I can have had any opportunity of hearing of.

Upon the whole, there is not perhaps an example, in all the history of philosophy, of so much zeal and emulation being excited by any object. I even question whether the subject of *electricity*, under the auspices of Dr. Franklin, ever engaged more general attention; and now these two pursuits are happily united, and admirably promote each other.

In reality, this is not now a business of *air* only, as it was at the first; but ap-
a 4
pears

pears to be of much greater magnitude and extent, so as to diffuse light upon the most *general principles* of natural knowledge, and especially those about which *chymistry* is particularly conversant. And it will not now be thought very assuming to say, that, by working in a tub of water, or a basin of quicksilver, we may perhaps discover principles of more extensive influence than even that of *gravity* itself, the discovery of which, in its full extent, contributed so much to immortalize the name of Newton.

Having been the means of bringing so many champions into the field, I shall with peculiar pleasure attend to all their achievements, in order to prepare myself, as I promised in the preface to my last volume, for writing the *history* of the campaign; and I trust that all my brethren in the science will have confidence in my justice to their respective merits.

I flatter myself, that the very frank and candid manner in which I have related what I have done myself, will procure me sufficient

cient credit for my impartiality with respect to others. It will be very evident, that I have left myself hardly any other merit than that of *patient industry* and *attention*, and that of keeping my mind so far detached from the influence of prejudice, as to be able to pursue fairly such casual observations as presented themselves to me.

There is nothing capital in this volume from which I can hope to derive any other kind of honour, than that of being the instrument in the hands of divine providence, which makes use of human industry to strike out, and diffuse, that knowledge of the system of nature, which seems, for some great purpose that we cannot as yet fully comprehend, to have been reserved for this age of the world; concerning which I threw out some farther hints in my former preface, which the excellent French translator was not permitted to insert in his version.

I even think that I may flatter myself so much, if it be any flattery, as to say, that there is no history of experiments more truly *ingenuous* than mine, and especially

cially the Section on the discovery of dephlogisticated air, which is the most important in the volume. I am not conscious to myself of having concealed the least hint that was suggested to me by any person whatever, any kind of assistance that has been given me, or any views or hypotheses by which the experiments were directed, whether they were verified by the result, or not.

In this volume the reader will find much light thrown upon many things which were inexplicable to me when I published the former volume; but, on the other hand, there are many things, in this, as inexplicable to me now as the others were before; and for the elucidation of them we must wait for more experiments, and more discoveries.

As, in the preface to my former volume, I quoted a very striking observation of Father Beccaria, I shall, in this, present my reader with a quotation from another Italian philosopher, the Abbé Fontana, which is as much to my present purpose.

“ Le

“ Le fisiche ricerche cominciate in questi
 “ ultimi anni con tanto successo dai filo-
 “ sofi, forse per mera curiosità, sopra le
 “ diverse qualità e indole dell’ aria natu-
 “ rale e fattizia, potrebbero in breve diven-
 “ tare di somma importanza. E par’ che
 “ già ci avviciniamo ad una di quelle gran-
 “ di epoche, che la natura conduce, dopo
 “ un lasso di secoli, e che marca con qual-
 “ che grande scoperta, per la felicità del
 “ genere umano.” *Ricerche Fisiche*, p. 21.

In English.

“ The inquiries that have lately been
 “ so successfully begun by philosophers,
 “ perhaps through mere curiosity, into the
 “ properties of air, natural and facti-
 “ tious, may soon come to be of the
 “ greatest importance. And we seem to
 “ be already approaching to one of those
 “ great epocha’s, to which nature con-
 “ ducts us, after a lapse of ages, and
 “ which she distinguishes by some great
 “ discovery, for the benefit of mankind.”

To this second volume I have added a paper published in the 60th volume of the Philosophical Transactions, on the *conducting power of charcoal*, because it has a near relation to the subject of *air*; and because it contains an account of many new facts, of a very remarkable nature, to which I wish to draw the particular attention of philosophers and chymists.

I have also inserted the substance of the pamphlet on *the impregnation of water with fixed air*, having no intention to publish it any more separately; prefixing to it a history of matters relating to it, and subjoining to it a comparison of this method with another that has been invented since, for the same purpose. I have also added an *alphabetical index* to both the volumes.

I am very sorry to have had occasion to insert in this volume a particular section on the *mistakes* that have been made, with respect to my Observations and Experiments, by several foreign philosophers. But they are so many, and so gross, and made by persons of so much reputation,
that

that I have thought it necessary to do so, both on my own account, and also to obviate such misrepresentations of *facts*, as might retard the progress of philosophical knowledge.

For these mistakes foreigners may plead the want of a perfect knowledge of the English language; and, in some measure, the plea may be admitted, though every person should take care to make himself fully acquainted with what he proposes not only to understand, but to explain to others.

I imagine, however, that both Mr. Lavoisier, and Sig. Landriani, took their accounts of my Experiments not from my own work in English, but from some preceding translations into French, and Italian; taking for granted that they were exact. Sig. Landriani, I am confident, understands English very well. He has lately informed me, that he will undertake the translation of all that I have written on the subject of air; and he will, I doubt not, both do justice to me, and credit to himself. Mr. Gibelin, who has acquitted him-

self so well in the French translation of the first volume of this work, has undertaken the second. I have, at his request, already sent him the printed sheets, and I believe he will dispatch the work with all convenient expedition. I have also reason to think, that the translation of this work into High Dutch, by Dr. Ludewig of Leipzig, will be very accurate. Upon the whole, therefore, I flatter myself that, for the future, my sense will be fairly represented; and perhaps with more accuracy, than if the mistakes I have been obliged to animadvert upon had not been made.

I wish that I could make as good an apology for Mr. B. Wilson, as for the foreign philosophers above-mentioned. This gentleman, in his late *Treatise on phosphori*, has animadverted upon me for speaking, in my *History of discoveries relating to vision, light, and colours*, of paper being *red hot*, and *cooled again*; when, in the printed *errata* of the book, he would have found, “for *red hot*, read *very hot*.” This was a mistake of my amanuensis,
and

and I thought it would be sufficiently rectified, by inserting it in the *errata*. I should certainly have reprinted the leaf, if I could have suspected that such an use would have been made of it. Before this gentleman points out any more of my mistakes, I hope he will take the trouble to see whether I have not noted them myself; or, if his copy of my books should happen to want the printed *errata*, that he will supply the place of it with a little *candour*. His paragraph relating to it, p. 10, is as follows.

“ Dr. Priestley, in his *History of Beccari's discoveries*, has mentioned a very remarkable experiment. He tells us, that Beccari found that paper, after it had been made *red hot*, and *cooled again*, was an excellent phosphorus. I must own that, upon the strictest research into the work to which he refers, I have not been able to find any such account. Nor do I conceive in what manner paper can be made *red hot*, and afterwards *cooled*, without being reduced to ashes.

“ I should

“ I should, nevertheless, be greatly obliged
“ to the learned historian, who relates the
“ experiment, for an explanation of his
“ meaning, if he can point out the pas-
“ sage to which his elaborate work re-
“ fers.”

Besides, except that the words *red hot* were not in the treatise I was abridging, I have nothing to alter with respect to it. For if Mr. Wilson does not know it at present, he may satisfy himself in half a minute, that white paper always becomes *red* by heat, before it is turned *black*.

Having this occasion to mention Mr. Wilson and his book, which I think to be, in several respects, a very valuable one, I must farther observe, that he takes every opportunity of cavilling at my *History*; when, admitting his pretended discoveries, which were subsequent to the publication of that work, it is not liable to the least just exception: since, as an *historian*, I could not but take for granted,
that

that there was no fallacy in the experiments of Mr. Canton and Father Beccaria, especially as they confirm the Newtonian doctrine concerning light.

Father Beccaria had advanced, that the Bolognian phosphorus emits the very same particles of light which it had imbibed; so that, if red rays only had been thrown upon it, it would appear red, and if it had been exposed to blue rays only, it would appear blue, &c. *Phil. Transf.* vol. 61: p. 212. Mr. Wilson endeavoured to repeat these experiments, but without success; and because, in the manner in which he made them, all his phosphori appeared of the same colour, he concludes, contrary to what Mr. Canton and Father Beccaria had supposed, that the light which the phosphorus emits, is not the same that it had imbibed; but that there is a transmutation of the inflammable principle of the phosphorus itself into light.

I have not endeavoured to ascertain this fact, not having, as yet, any convenience for experiments of that kind; but I will take the liberty to say, that a philosopher
b of

of such a class as Father Beccaria, is intitled to the greatest respect; and that his conclusions should not have been controverted, but upon much better grounds than Mr. Wilson's. For, from the manner in which his experiments were made, I cannot but think them to have been inadequate to the object of them, and that they must be considered as indecisive. And whenever the experiments shall be made with a stronger and purer light, I have very little doubt but that Father Beccaria will appear to have been in this, as well as in all his other numerous experiments, perfectly accurate, and that the conclusion which he draws from them is strictly just, though contradicted by Mr. Wilson. In this, however, I may be mistaken.

Having proceeded thus far in an account of the misrepresentations of my meaning, advancing from a less to a greater cause of complaint, from simple admonition to reprehension, I shall go one step farther, to take notice of a wilful and most wicked perversion of my meaning, in a business of much more importance than those which I have mentioned already. If it be
said

said that in this I digress too far, let it be considered that, in a *preface*, authors have always claimed a right of saying whatever they pleased concerning themselves; and not to lose this right, it must now and then be exercised. It will be seen, however, that, in this digression, I have views not very foreign to the subject of a treatise addressed to philosophers.

Notwithstanding my studies and writings are chiefly of a theological nature, and my philosophical pursuits only occasional; notwithstanding, in my *Institutes of natural and revealed religion*, I have an intire volume on the evidences of christianity, in which I flatter myself I have placed several parts of it in a new and stronger light, and this from *inclination* only, without a shadow of *interest* to bias me, I have been represented in an artful *advertisement*, frequently repeated in all the English newspapers, as not believing in a future state. The author of the advertisement has, for this base purpose, quoted the following mutilated sentence from an *Essay* of mine prefixed to

b 2

my

my edition of *Dr. Hartley's Observations on the human mind*, p. 20.

“ I am rather inclined to think, though
 “ the subject is beyond our comprehension
 “ at present, that man doth not consist of
 “ two principles so essentially different
 “ from one another as *matter* and *spirit*,
 “ which are always described as having
 “ no one common property, by means of
 “ which they can affect or act upon each
 “ other, &c. I rather think that the whole
 “ man is of some uniform composition,
 “ and that the property of *perception*, as
 “ well as the other powers that are termed
 “ *mental*, is the result (whether necessary
 “ or not) of such an organical structure
 “ as that of the brain. Consequently that
 “ the whole man becomes extinct at death,
 “ &c.”

The wickedness of this representation
 will appear by reciting the remainder of
 the sentence.

——“ at death, and that we have no
 “ hope of surviving the grave, but what
 “ is

“ is derived from the scheme of revelation.”

In the same page I also observe that, though this doctrine favours the opinion of the lower animals differing from us in *degree* only, and not in *kind*, “ it does not necessarily draw after it the belief of their surviving death as well as ourselves ; this privilege being derived to us by a *positive constitution*, and depending upon the promise of God, communicated by express revelation to man.”

This affair has been the occasion of much exultation among *bigots*; as a proof that freedom of thinking in matters of religion leads to infidelity ; and *unbelievers*, who have never read any but my philosophical writings, have considered me as one of their fraternity. To the former I shall say nothing, because it would avail nothing. To the latter, of whom I have more hopes, I would take this opportunity of observing (and in this I address myself to foreigners more than my own countrymen) that, as they will agree with me in

the opinion of the *natural mortality of the soul*, which is agreeable to every appearance in nature, it nearly concerns us to consider whether there be no evidence of a future life of retribution independent of the contrary doctrine, which has no countenance from the scriptures *; that it argues extreme narrowness of mind, unworthy of the spirit of philosophy, not to extend our views and inquiries beyond the circle of those objects about which natural philosophy is conversant, which terminate in gaining a knowledge of the visible system of nature; and that it behoves us to consider whether the great Author of nature has not afforded us sufficient *data* for knowledge infinitely more interesting to us, more immediately respecting our re-

* In this opinion I am far from being singular. It is known to have been the opinion of Luther, and many of the most eminent of the first reformers. Of late years it has been most ably supported by the present excellent Bishop of Carlisle, and is now generally adopted by rational christians. The opinion of the *natural immortality of the soul*, had its origin in the heathen philosophy; and having, with other pagan notions, insinuated itself into christianity (which has been miserably depraved by this means) has been the great support of the popish doctrines of *purgatory*, and the *worship of the dead*.

lation

lation to himself, and his gracious provision for our improvement and happiness, not only in this *infancy of our being*, but to a period which has no bounds.

Let philosophers, as certainly becomes their character, consider *facts*, and the *phenomena of the human mind*, as influenced by facts, and it must appear to them to be utterly incredible, that christianity should have arisen, have been propagated, and have established itself in the world, in the circumstances in which all history shews that it did arise, and was propagated, if it had not been founded on truth and fact; such facts as are strictly the subject of historical investigation.

The common objection against religion among philosophers is, that it was invented by artful interested *priests*, or wise *magistrates*: but it is not fact that *christianity* had any such origin. No priest was concerned in the invention of it, nor did any civil magistrate foster it: but, on the contrary, it was violently opposed by all priests, and all magistrates, wherever it ap-

b 4

peared,

peared, and by its own evidence it triumphed over both. These are *facts* worthy of the attention of philosophers, as such.

To quit this subject for that with which I began this preface, and in which I shall be more attended to by philosophers in general, I would caution my reader not to be too sanguine in his expectations from the happy train which this branch of philosophy seems to be in. Considering the unexampled rapidity with which discoveries have hitherto been made in it, the number of persons in many and distant countries now engaged in these pursuits, and the emulation that is necessarily excited in such circumstances; and considering, at the same time, how nearly this subject is allied to the most general and comprehensive laws of nature with which we are acquainted; some may be apt to imagine, that every year must produce discoveries equal to all that were made by a Newton or a Boyle; and I am far from saying that this may not be the case, or that it is very improbable.

But,

But, though I have little doubt, from the train that things are visibly in, that philosophical discoveries in general will go on with an accelerated progress (as indeed they have done ever since the revival of letters in Europe) it would be too rash to infer, from any present flattering appearances, that any particular expedition into the undiscovered regions of science will be crowned with more distinguished success than another. Nothing is more common, in the history of all the branches of experimental philosophy, than the most unexpected revolutions of good or bad success. In general, indeed, when numbers of ingenious men apply themselves to one subject, that has been *well opened*, the investigation proceeds happily and equably. But, as in the history of *electricity*, and now in the discoveries relating to *air*, light has burst out from the most unexpected quarters, in consequence of which the greatest masters of science have been obliged to recommence their studies, from new and simpler elements; so it is also not uncommon for a branch of sci-

ence to receive a check, even in the most rapid and promising state of its growth.

Judging, however, from my present views of the subject, I am willing to hope that the beginning of this preface will not contribute to raise too high expectations. The *incomplete experiments*, indicated in the course of both these volumes, and especially in the second, will, alone, furnish matter for, at least, as much experimental investigation as all that I have yet gone through; and I need not tell the real philosopher, that many of them are of such a nature, as promise to reward the sagacious experimenter with the most important discoveries, as they evidently border upon, and may lead to, much greater things than any that I have hitherto investigated; and my *hints* for other new experiments, which I have not thought it worth while to trouble the reader with at this time, are more than I have ever had before me since I began these inquiries. From this I think I may reasonably infer, that the subject is so far from being *exhausted*, that the most that can be said of it is,

is, that it is *pretty well opened*, so as to exhibit an inviting prospect to future investigation.

To accelerate this investigation, I have hitherto made the most early publication of my observations, and have concealed from no person whatever any thing that has occurred to me; and though this conduct has exposed me to some inconvenience, I am not yet discouraged; but, whoever may avail themselves of it, I shall, for some time longer, at least, and I hope through life, persist in the same habit of the most open and unreserved communication, private and public.

I have not in this volume, as in the former, a section of *conjectures, speculations*, and *hints*, because I have not yet sufficiently reflected upon the *facts* that suggest them. The facts, however, will furnish abundant matter to those who are disposed to speculate, and especially on the subject of the mutual convertibility, and ultimate identity, of all the acids when combined with substances in the form of

air; but I chuse to wait for more facts, before I deduce any general theory. In the mean time the field is as open to others as to myself.

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T H E

presence, it was absolutely impossible that I should have done otherwise, without being very tedious, and even appearing ridiculously trifling, to those who were at all versed in things of this nature. And though I am willing to sacrifice a great deal to the desire that I have to facilitate these inquiries to beginners; yet as I do not, in these volumes, pretend to compose an *elementary treatise*, for the use of those who have no previous knowledge of the subject (but, beginning where others have left it, to resume the inquiry, and extend the bounds of our knowledge relating to it) propriety requires that I do not sacrifice *too much* to so foreign an object. Besides, that readiness and certainty in the use of instruments, which is acquired by experience, cannot be communicated by any verbal instruction, but must be the result of much practice, with respect to others, as it was with myself; and a variety of subsidiary helps, which contribute much to the facility and elegance of operating, will so certainly occur to any person who shall actually go to work in this business, that it is altogether unnecessary to enter into a detail of them.

Besides, every man will, in many things, have a method of his own; so that two persons, who should do the very same things, would

would fall into different methods of doing them, and it is probable that each of them would fancy that there was a peculiar advantage in his own. Learning, however, as I profess I always do, to an inclination to gratify beginners, and to give them all the assistance in my power, I shall be as particular, as with propriety I can be, in the description of the principal instruments, and mode of operating, which I have made use of in my late experiments.

The figures *a, a, a*, represent *phials*, of which I have made very great use in the whole course of my experiments. They are made round, and very thin at the bottom, and the mouth is ground smooth; so that they may be either used with a cork, or, being filled with quicksilver, or any other fluid, will stand firm when inverted, and placed upright, in basons containing the same fluid. When they are used with corks, like common phials, they will bear the application of a pretty sudden heat from the flame of a candle, or otherwise, which the common phials, being generally thickest at the bottom, will not bear; and therefore, before I got these phials, I used to grind the bottoms of the common phials very thin: but I have found a very great convenience in having these made thin on purpose;

pose; and besides, their being round at the bottom, is, on many accounts, a great advantage.

Without vessels of this form, it is hardly possible to extract air from any substance confined by quicksilver, which is an operation that the reader will find, in the course of this volume, I have made very great use of; but nothing is easier in such vessels as these: for standing with their mouths downwards, and the substances on which the experiment is made lying on the surface of the quicksilver, just under the thinnest part of the glass, it is very easy to present them to the focus of the burning lens, in such a manner that they shall be exposed to all the power of it, without breaking the glass. Care, however, must be taken, to place them short of the focus at first, that the greatest degree of heat may not be communicated at once. In most cases this moderate heat will be sufficient to produce a considerable quantity of air; and as there will then be a space void of every thing but air between the glass and the substance on which the heat is to be thrown, the greatest heat that the lens can produce may be directed upon it; since the glass through which the rays are transmitted, being at some distance
from

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from the focus, is in no danger of being broken or melted.

A skilful operator will be able to fill his vessel with the newly generated air by this means; but, in general, he will do well to content himself with getting it half-full, or less; for as the glass is necessarily thicker towards the mouth, there will be some danger of breaking it when the rays are transmitted near that place, and of losing the air that has been, perhaps, with great trouble and difficulty, procured. This has frequently happened to myself, and does so still every now and then, long accustomed as I have been to the operation.

If the substance on which the experiment is made be in the form of a *powder*, as red lead, and even many very light substances, it will be most convenient to put them into the vessel first; and the quicksilver may, with care, be poured upon them afterwards, so as to keep the substance at the bottom; and yet, when the vessel is inverted, it will remain at the top. When the light matter will not lie close, it will not be difficult, sometimes, to intercept it in the strait part of the phial, at the neck; but it will often be most convenient to
form

form these light matters into small balls, and put them into the vessel, through the quicksilver, with which it has been previously filled.

I would observe, with respect to this process, and every other in which vessels are to be filled with quicksilver, and then to be placed inverted in basons of the same, that no operation is easier (unless the mouth of the vessel be exceedingly wide) when the mouth of it is covered with soft leather, and, if necessary, tied on with a string, before it be turned upside down; and the leather may be drawn from under it when it is plunged in the quicksilver. If the mouths of the vessels be very narrow, it will be sufficient, and most convenient, to cover them with the end of one's finger.

In this process, there remains less doubt of the generated air coming from the materials on which the experiment is made, than when the focus of the lens is thrown upon them *in vacuo*; because there will often be room to suspect that common air may get into the receiver, in the course of a long process, at some place not sufficiently guarded; and besides, it is a great satisfaction to *see* the quantity of air that is generated at any particular time, during the course of a process; that the operator

rator may stop when he sees he has got a quantity sufficient for his purpose: whereas, unless he has a gage connected with his transferer (which may be inconvenient) he must admit water into his receiver before he can certainly tell whether he has procured any air, or not; and then it will be liable to be affected by the water, or by the air contained in the water, and which will be set loose very copiously on its first admission into the receiver.

But if the air, disengaged from any substance, will be attracted by mercury, as is the case with all those which contain the nitrous acid, this process cannot be used, and recourse must be had to the vacuum; and for this purpose it is necessary that the operator be provided with receivers made very thin, on purpose for these experiments. Such as are commonly used for other experiments are much too thick for this purpose, being very liable to break with the application of the heat produced by the burning lens. In this process, care should be taken to place the materials on glass, a piece of crucible, or some other substance that is known to yield no air by heat.

The figure *b*, represents a common glass phial with a ground stopple, with many small holes in it, which was a happy contrivance of
my

my ingenious pupil and friend Mr. Benjamin Vaughan. It is of excellent use to convey any liquid, or even any kind of air, contained in it, through the water, into a jar standing with its mouth inverted in it, without admitting any mixture of the common air, or even of the water; and yet the air generated within it has a sufficient out-let. These phials will be found useful in a great variety of experiments.

The figure *c*, represents a phial of the same form with *a*; but the neck is thicker, in order to be fitted with a ground stopple, perforated, and drawn out into a tube, to be used instead of the phial *e*, vol. I. plate 1. Till I hit upon this contrivance, which was executed for me by the direction of Mr. Parker, I had a great deal of trouble in perforating common corks, bending and fitting tubes to them; and, after all, the corks themselves, or the cement, with which I generally found it convenient to cover the ends of the tubes, were apt to give way, and to be the occasion of very disagreeable accidents. Besides, if any hot acid was used, the vapour would corrode the cork, and an allowance was to be made for the effect of that circumstance on the air: whereas, with this apparatus, which is exceedingly convenient and elegant, the opera-

tor may be sure that nothing but glass is contiguous to the materials he works upon, as he can perfectly exclude every other foreign influence; and while it remains unbroken, it is never out of repair, or unfit for use.

For many purposes, however, the former method, with corks and tubes, will be found very sufficient, and much less expensive; especially with the *fluor acid*; which corrodes glass, and which will presently eat through one of these delicate phials. For this purpose, therefore, I would recommend the use of a common and very thick phial, especially as no great degree of heat, and never any sudden application of heat is wanted.

The phial *c*, will be found sufficient for any purpose that does not require more heat than the flame of a candle held close to the bottom of it, can supply: but if there be occasion to place the phial in a sand-heat, and consequently if it must be put into a crucible placed on the fire, it will be necessary to have the tube, in which the ground stopple terminates, made as long as may be, as represented by *e*; otherwise the vessels that receive the air will be too near the fire. Nine or twelve inches,
d however,

however, will be a sufficient length for any purpose.

I have great reason to congratulate myself on this apparatus, having found it to be of most admirable use. For, in experiments with air; where the greatest possible accuracy is required, *lutes* are by no means to be trusted, since a variety of vapours, coming into contact with them, are considerably affected; whereas these stopples being ground air-tight, the operator may be perfectly at ease, both with respect to the quantity and the quality of his produce. To express this process as concisely as possible, I generally allude to it, by saying that the phials have *ground stopples and tubes*.

In experiments in which it is not worth while to be at the expence of these phials with ground-stopples and tubes, and yet where gun-barrels cannot be trusted to, on account of the materials corroding the iron, I have recourse to a kind of long phial, or a tube made narrower at the open end, nine or twelve inches in length, and of an equal thickness throughout, represented fig. *d*. When these phials are put into a crucible with sand, the bottom may be made red-hot, while the top is so cool, that a common cork (into which a glass-tube is inserted) will not be affected by the heat. In
fact,

fact, this vessel is a kind of gun-barrel made of glass, and is used exactly like the gun-barrel, except that it is not exposed to so great a degree of heat.

When the materials are put into this vessel, it must be filled up to the mouth with fine sand, that will give no air by the application of heat, and the cork must be thrust down close upon the sand. The air must be received as in plate 2. fig. 7. vol. I. Could this glass-vessel bear as great a degree of heat, and as suddenly applied as the gun-barrel, it would be an excellent instrument indeed. I have sometimes thought of getting them made of that kind of clay which is used for making crucibles; but these of glass have been generally sufficient for my purpose.

When a person has a great many trials to make of the goodness of air, it is of no small importance to have contrivances by which he may save time. Having, particularly, had frequent occasion to measure the purity of air by means of nitrous air, in which it is sometimes necessary to put several measures of one kind to one measure of the other; and being wearied with taking all the measures separately, at length I hit upon the very useful expedient of having the measures ready-made, consisting

sisting of vessels, the capacities of which had a known proportion to each other, as f, f, f , each vessel holding twice as much as the size next less than it. It is likewise convenient to have the vessels in which the mixture of air is made, fig. g , marked in a manner corresponding to these phials, that the diminution of the air may be perceived at once, without the application of any measure. If one of these phials contain an ounce-measure, and the rest be multiples and subdivisions of it, it will be still more convenient.

When the quantities of air to be measured are very small, phials will be too large. I have therefore a set of small tubes, b, b, b , bearing the same proportion to each other with the phials, the smallest of which contains very little indeed; and likewise a longer tube, i , marked in a corresponding manner, in which to mix the air contained in those tubes.

SECTION

S E C T I O N I.

Of VITRIOLIC Acid Air.

I Had no sooner exhibited the *marine acid* in the form of air, than it occurred to me that it might be possible to exhibit the other acids also in the same elegant manner, divested of the water with which they had hitherto been combined, and which must necessarily have been a great obstruction to the discovery of their real natures and affinities; but not being a practical Chemist, and living in the country, where I had no access to any person of that profession, and indeed not being sufficiently able to explain my wants, I met with many hindrances in the prosecution of my inquiries into this subject.

My first scheme was to endeavour to get the *vitriolic acid* in the form of air, thinking
 B that

that it would probably be easy to confine it by quicksilver; for as to the nitrous acid, its affinity with quicksilver is so great, that I despaired of being able to confine it to any purpose, as I have observed in the former volume, p. 273. I therefore wrote to my friend Mr. Lane, to procure me a quantity of *volatile vitriolic acid*, which is the common vitriolic acid combined with phlogiston, at the time that I was intent upon the prosecution of my former experiments; but by some mistake of my meaning, a different thing from what I intended was sent me.

Seeing Mr. Lane the winter following, he told me that if I would only heat any oily or greasy matter with oil of vitriol, I should certainly make it the very thing I wanted, viz. the *volatile*, or *sulphureous vitriolic acid*; and accordingly I meant to have proceeded upon this hint, but was prevented from pursuing it, by a variety of engagements, till after the publication of my late treatise.

Some time after this, I was in company with Lord Shelburne at the seat of Mons. Trudaine, at Montigny in France, where, with that generous and liberal spirit by which that nobleman is distinguished, he has a complete apparatus of philosophical instruments,
with

with every other convenience and assistance for pursuing such philosophical inquiries as any of his numerous guests shall chuse to entertain themselves with. In this agreeable retreat I met with that eminent philosopher and chymist, Mons. Montigni, Member of the Royal Academy of Sciences; and conversing with him upon this subject, he proposed our trying to convert oil of vitriol into vapour, by boiling it with a pan of charcoal in a cracked phial. This scheme not answering our purpose, he next proposed our heating it together with oil of turpentine. Accordingly we went to work upon it, and soon produced a quantity of some kind of air confined by quicksilver; but our recipient being overturned by the suddenness of the production of air, we were not able to catch any more than the first produce, which was little else than the common air which had lodged on the surface of the liquor, and which appeared to be a little phlogisticated, by its not being much affected by a mixture of nitrous air.

Having no opportunity of repeating the experiment at that time, I did nothing with a view to it till my return to England; when, on the 26th of November, 1774, I resumed

the operation, beginning with *olive oil*, and by the help of a more convenient kind of glass vessel, represented fig. *a*, which I had procured for these and other similar purposes, I found very little difficulty in the prosecution of the experiments.

As I wish that my reader may enjoy the benefit of my experience, I would caution him, if he chuse to repeat the experiments, not to put too much oil, or any other similar substance, to the oil of vitriol, in order to produce this air. I began with using about one fifth part of common oil, leaving space enough, as I thought, in the phial, for the ebullition that might be occasioned by the production of air; but as soon as the vessel was heated to a certain degree, the production of air was exceedingly rapid; and though I withdrew the candle which I had applied to it for that purpose, the ebullition continued to increase, till, the capacity of the tube not being sufficient for the transmission of the generated air, the cork was driven out of the phial, and all the contents of it exploded.

After this I only slightly covered the spirit of vitriol in the phial with olive oil, and then the phenomena were similar to those in the former experiment, at the same time that the
process

process was more manageable ; for, by applying or withdrawing the candle, as I saw occasion, I got what quantity of air I pleased ; and removing the phial, in this state of ebullition, from one vessel to another, I filled several of them with this new species of air, as easily as I had been used to do it with the marine acid air ; and the whole process was as pleasing and as elegant. Indeed, this manner of producing air from substances contained in small phials, and receiving the produce in quicksilver, when it is of such a nature that it cannot be confined by water, has never failed to strike every person to whom I have shewed it.

The moment that I saw the acid of vitriol assume the form of air by the addition of phlogiston, I concluded that the marine acid also must have been capable of being exhibited in the same manner, by means of the phlogiston which it naturally contains, and which is inseparable from it ; and moreover, that, probably, some portion of phlogiston may be necessary to the volatility and elasticity of all substances whatever ; so that the marine acid air may not be precisely what I had before imagined, viz. the *pure marine acid in the form of air* ; but that, though it is by this means exhibited free from water, which, in a

variety of respects, modifies and restrains its action upon various bodies, it is still combined with a portion of phlogiston. Since, however, all the bodies with which we are acquainted are, in some degree, elastic, being capable, at least, of being condensed by cold, and dilated by heat, it may not be possible to separate this principle intirely from any substance in nature; and therefore, in a sense sufficiently near the truth, it may still be said that the marine acid air is nothing but the marine acid; the phlogiston it contains being so small, as not to be discoverable by any of the usual tests of its presence.

Before any air is produced from the mixture of inflammable matter and oil of vitriol, the whole quantity becomes very black; and a quantity of this spirit, thus impregnated with phlogiston, will yield many times more air than an equal quantity of the strongest spirit of salt: but I never measured it with any exactness.

When the vitriolic acid air is produced in great plenty, the top of the phial in which it is generated is generally filled with white vapours. This air has also the same appearance as it is transmitted through the glass tube, and it is sometimes discoverable in the recipient.

Vitriolic acid air is equally transparent with marine acid air, and seems to have no more affinity with quicksilver; for when confined by quicksilver, the dimensions of it are not liable to any variation, excepting by heat and cold, just like common air; provided there be no moisture in the recipient, or in the quicksilver. As the resemblance between these two acid airs was so great, it was natural for me to have a view to the experiments I had made with the marine acid air, in conducting these that relate to the vitriolic acid, which the reader will easily perceive.

Water being admitted to the vitriolic acid air absorbed it about as readily as the marine acid air; and by its union with it must have formed the volatile or sulphureous acid of vitriol. Indeed the result of this combination was so obvious, that I did not think it necessary to make the experiment.

Like the marine acid air, this vitriolic acid air extinguishes a candle, but without any peculiar appearance in the colour of the flame, as it goes out, or as it is lighted again, which is observable when the experiment is made with the marine acid air. Vitriolic acid air is also heavier than common air; for a candle being let down into a vessel filled with it, was

extinguished many times successively, and even after it had stood a full hour with its mouth exposed to the common air.

Ice is instantly dissolved in this, as well as in the marine acid air, and the water impregnated with it continues to dissolve more ice. Upon this occasion I observed that this acid air bears to be exposed to cold, without any greater diminution of its bulk than common air is subject to in the same circumstances; which appears to me to be a sufficiently proper criterion to distinguish *air* from *vapour*. In a certain degree of heat, indeed, even *water* may be exhibited in the form of air; but it is a degree of heat that far exceeds what is usual in our atmosphere; and in other cases terms are applied to very great use, for the distinction of bodies, which, if examined with strictness, would be found ultimately to run into one another, the difference between them being in *degree* rather than in *kind*: but a very *great difference* in degree affords a sufficient foundation for a difference in appellation.

The phenomena which attend the mixing of alkaline air with the marine acid air, were so striking, that I had not been many hours in possession of the vitriolic acid air without trying whether the effect of the same mixture with
this

this acid air would not make a similar appearance, and the experiment fully answered my expectations. A like beautiful white cloud was formed the moment that these two kinds of air came into contact, the quantity of air was diminished as fast as the alkaline air was admitted, and the quicksilver rose almost to the top of the receiver.

I observed also, that when I put the alkaline air to the vitriolic acid air, the white cloud rose immediately to the top of the vessel, as in the experiment with the marine acid air; which proves that the alkaline air is, in both cases, the lighter of the two. In both cases also, if the alkaline air be produced first, the acid air being admitted to it, forms a cloud which rests upon the quicksilver; never extending beyond a very small space, and rising only as the quicksilver rises. The substance that is formed by the union of the alkaline air with the vitriolic acid air, must necessarily be the *vitriolic sal ammoniac*; but I made no experiment to ascertain it. The quantity of this salt with which my receivers are coated in these experiments is readily dissolved in water, as in the experiments with the marine acid air. This, however, it will be seen, is not the case with the salt that is formed by another of the acid airs with alkaline air.

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The mixture of other kinds of air with vitriolic acid air produced no remarkable appearance whatever. When, however, I had put a quantity of this acid air to a quantity of common air, in order to observe whether the former might not part with some of its phlogiston to the latter, though I perceived no immediate diminution of the bulk of air, as in the mixture of nitrous and common air; yet when they had continued together two days, and water being admitted to the mixture had absorbed the acid air, the common air which remained appeared, by the test of nitrous air, to be considerably injured; so that the vitriolic acid air must have communicated some of its phlogiston, which is an effect that is not produced by the marine acid air when mixed with common air. What effect the vitriolic acid air would have had upon other kinds of air, had they continued together a longer time, I cannot tell.

A quantity of this acid air mixed with inflammable air stood some hours; but when water had been admitted to them, I could not perceive either that the quantity of inflammable air was altered, or that its inflammability was in the least impaired.

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I once put equal quantities of marine and vitriolic acid air into the same receiver, and observed that they mixed without exhibiting any appearance whatever; and when alkaline air was admitted to them, the appearance was the same as if it had been admitted to either of them singly, the white cloud rising instantly to the top of the vessel. Had I, after the experiment, examined the *salts* which adhered to different parts of the inside of the vessel, I might perhaps have discovered which of the two acid airs was specifically lighter than the other; but I suspect that they were intimately mixed, and therefore that the salt was some uniform composition, between the common and the vitriolic sal ammoniac.

I thought it rather extraordinary, that whereas the marine acid, which is reckoned the weakest of all the three mineral acids, should, when exhibited in the form of air, be able to dislodge both the vitriolic and the nitrous acids from several of their bases; yet that this vitriolic acid, which is reckoned the strongest of the three, when seemingly exhibited to equal advantage, by being divested of the water with which it is usually combined, should not, in any instance in which I made the experiment, dislodge either of the other acids from any basis with which they were united. *Nitre,*
common

common salt, and *sal ammoniac*, were all introduced to this air, without either affecting it, or being affected by it.

Vitriolic ether imbibes vitriolic acid air as readily as water imbibes it. The ether, however, was soon saturated with it, and afterwards was, to all appearance, both as transparent, and as inflammable as before.

A piece of *phosphorus* remained a day and two nights in vitriolic acid air, without sensibly affecting it. It gave no light in this air; but the upper surface of it turned black, and the surface of the quicksilver on which it lay, had a deep yellow or blackish kind of scum upon it, as if it had been in part dissolved by the acid.

Iron is readily dissolved in marine acid air, but is not at all affected by the vitriolic acid air; though, when combined with water, it is so powerful a menstruum for iron. But this, indeed, is the less extraordinary, as this acid ceases to affect iron when it is strongly concentrated. I kept a number of iron nails in vitriolic acid air two days, without any sensible effect either upon the air, or the nails. There was no appearance of their being in the least corroded.

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A piece of *liver of sulphur*, in three days, absorbed the whole of a quantity of this kind of air, without sensibly affecting the colour or appearance of the liver of sulphur.

Charcoal, which forms inflammable air, by being introduced to marine acid air, only absorbs the vitriolic acid air; which, however, it does pretty rapidly, and acquires a pungent smell from being exposed to it, without producing any other effect that I could perceive. I made several pieces of charcoal imbibe as much of this acid air as they could; but, after this, fresh pieces absorbed the remainder, so that the air had only been, as it were, condensed on its surface. This I have observed to be the case with alkaline air, and in some experiments with other kinds of air which cannot be confined but by quicksilver; and I do not clearly understand it. The charcoal, in this experiment, was made very dry, or it might have been suspected that the moisture adhering to it had absorbed the air.

Vitriolic acid air dissolved *camphor* pretty readily, and reduced it to a transparent liquor. Water being admitted to it, the camphor reassumed its natural solid form, but seemed to have acquired an acidity in its taste.

I have noticed a very remarkable effect of alkaline air upon alum, rendering it white and opake, as if it had been calcined, but without altering its figure. The same, to appearance, is the effect of vitriolic acid air upon *borax*. This substance absorbed a pretty large quantity of this air in two days. What remained of the air extinguished a candle. But this effect was probably owing to a small proportion of fixed air that was produced at the same time with the vitriolic acid air. I repeated this experiment with borax, and let the process continue three days, when the effect was precisely the same as before, the borax retaining its form, but being rendered white and opake. The acid air had, no doubt, seized upon the water which enters into its composition, as I conjecture to be the case with respect to alkaline air and alum.

As it is well known that the common vitriolic acid is changed into volatile or sulphureous acid of vitriol by fumes of charcoal, if the vessel in which it is heated has a crack in it, through which the fumes can have access to the acid, I had the curiosity to try whether the same effect would not be produced by heating the charcoal in the acid. Accordingly I put some bits of *charcoal* into my phial, instead of the oil, or other inflammable matter, which I had

had used before; and, applying the flame of a candle, I presently found that the vitriolic acid air was produced as well as in the former process, and in several respects more conveniently, the production of air being more equable; whereby the disagreeable effect of a sudden explosion is avoided.

It is necessary, however, that the charcoal be very well burned, so that all air be expelled from it; otherwise, there will be a mixture of fixed or inflammable air along with the acid air, especially when a considerable degree of heat is applied to produce the air. Having often got vitriolic acid air from charcoal, on account of the easy and equable production of it in this manner, I several times observed that there was a considerable residuum after it had been exposed to water: and once I found that the residuum made lime-water turbid; a sure sign of its containing fixed air.

When I endeavoured to procure this air by the same process from *ether*, about one half of the produce was permanent and inflammable. The oil of vitriol became perfectly black by this means, as in heating other phlogistic matters in it. Afterwards, heating the same oil of vitriol and ether, about one fourth of the produce only was inflammable; and had I con-

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tinued to use the same mixture, the produce would probably have been less and less inflammable, and more purely acid, every experiment.

Finding that a great variety of substances, containing phlogiston, enabled the oil of vitriol to throw out a permanent acid air, I had some suspicion that mere *heat* might do the same; but I did not find that there was any foundation for that suspicion. When, indeed, I put nothing into the phial along with the oil of vitriol, but only heated it in a phial closed with a common *cork*, air was produced pretty fast; but the cork was corroded, and the oil of vitriol was as black as if the cork had been dipped into it; so that phlogiston had evidently come from the cork. It was plain, however, that some acid vapour had risen, or the cork could not have been affected by it.

When, however, by the help of Mr. Parker, I got glass phials with ground stopples, perforated, and drawn out into tubes, such as are represented fig. c, I found that heating the oil of vitriol in them produced no air whatever; though, for a long time, I gave a small phial as much heat as I possibly could, by keeping it surrounded with the flames of two large candles. I was not able to make it actually boil with this heat, but a white vapour issued

issued from it, and circulated in the top of the phial, rising in one place, and being condensed in another.

But though I got no air from the oil of vitriol by this process; air was produced at the same time, in a manner that I little expected, and I paid pretty dearly for the discovery it occasioned. Despairing to get any air from the longer application of my candles; I withdrew them; but before I could disengage the phial from the vessel of quicksilver, a little of it passed through the tube into the hot acid; when, instantly, it was all filled with dense white fumes, a prodigious quantity of air was generated, the tube through which it was transmitted was broken into many pieces, (I suppose by the heat that was suddenly produced) and part of the hot acid being spilled upon my hand, burned it terribly, so that the effect of it is visible to this day. The inside of the phial was coated with a white saline substance, and the smell that issued from it was extremely suffocating.

This accident taught me what I am surprized I should not have suspected before, viz. that some *metals* will part with their phlogiston to hot oil of vitriol, and thereby convert it into a permanent elastic air, producing

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the very same effect with oil, charcoal, or any other inflammable substance.

Not discouraged by the disagreeable accident above-mentioned, the next day I put a little *quicksilver* into the phial with the ground stopple and tube, along with the oil of vitriol; when, long before it was boiling hot, air issued plentifully from it; and being received in a vessel of quicksilver, appeared to be genuine vitriolic acid air, exactly like that which I had procured before; being readily imbibed by water, and extinguishing a candle in the same manner as the other had done. A white salt was formed; but what I thought a little remarkable, was, that, whereas in all the former experiments the oil of vitriol turned black before it yielded any air; this was not the case here; for it continued colourless and transparent during the whole process.

After this I repeated the experiment with several other metals; but with a considerable variety in the results.

Putting pieces of *iron wire* into the oil of vitriol, a very small quantity of air was produced without heat; but this soon ceasing, I applied the candle, when, with a degree of heat, seemingly greater than that at which the
air

air had risen from the quicksilver in the same circumstances, air was produced in great plenty. When I had got about three ounce measures of it, I admitted water to it, and about four fifths of the whole was presently absorbed. The remainder was inflammable, burning very red.

Had the oil of vitriol been more concentrated, or had I continued the process longer, a greater proportion of the air would probably have been acid, and less of it inflammable. In this experiment the oil of vitriol became very opaque, being of a deep grey colour. The iron which had undergone this process, and which I had laid aside without any expectation, was, in a few days, covered with a whitish dust; and after it had been wiped clean, was covered again with the same matter. It is very much unlike the rusting of iron in other circumstances.

About one third of the produce of air from *zinc*, was acid, and the remainder inflammable. Indeed it was evident that the acid had a considerable effect upon the zinc before the application of the candle, small bubbles of air continually rising from it. The oil of vitriol, which had been used in this process,

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after

after a long time, deposited a white matter, which I suppose to be the *flowers of zinc*.

Copper, treated in the same manner, yielded air very freely, with about the same degree of heat that quicksilver had required, and the air continued to be generated with very little application of more heat. The whole produce was vitriolic acid air, and no part of it inflammable. The oil of vitriol remained a long time turbid, but at length deposited a brownish matter.

The solution of *silver* in the same manner, had the very same result, all the air being acid, and no part of it inflammable. The oil of vitriol acquired a kind of orange-colour, and deposited nothing.

With a very great degree of heat *lead* yielded a little air, which was wholly acid, and had nothing inflammable in it.

Gold yielded no air at all in this treatment; but the oil of vitriol acquired the same orange-colour that it had when the silver had been heated in it.

Neither had this treatment of *platina* any sensible effect. What I made use of was some
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which I had been favoured with from Dr. Irving carefully purged from iron.

In most of these processes, air seems to issue from the substances immediately upon the application of heat, and sometimes without it: and this first produce of air forms bubbles, which continue some time on the surface of the liquor. But it seems to be nothing more than the common air which had adhered to the surfaces of those substances, or had been confined in the little cavities near the surface, when they happened to be rough. For this seeming production of air soon ceases, and no more is produced without a much greater degree of heat; and when the genuine acid air begins to rise, bubbles formed by it break instantly, like bubbles of air in spirit of wine, and there is nothing like froth on the surface of the oil of vitriol.

As sulphur is formed by the union of phlogiston with oil of vitriol highly concentrated and very hot, I imagined that by heating substances containing phlogiston in vitriolic acid air, I could not fail to produce sulphur; but I tried charcoal in this manner without the effect that I had expected from it. The heat of a burning lens thrown upon it in this acid air, only made it throw out that quantity of the air, which, as I have observed before, is

condensed upon its surface, or imbibed by it. The air that was unabsoꝛbed after this operation was in part fixed, and in part inflammable, having come from the charcoal.

There was frequently, however, the appearance of sulphur produced upon the mixture of alkaline air with vitriolic acid air ; for the inside of the tube would be covered with a perfectly yellow matter. But this colour goes off in time, and nothing but a white saline substance remains. This yellow appearance I first observed when I had produced the vitriolic acid air from ether ; but afterwards I found the same effect when it was produced from charcoal, and still more remarkably when it had been produced from copper. Why this yellow colour should not be permanent, I do not understand.

SECTION II.

Of VEGETABLE Acid Air.

Having hit upon a method of exhibiting some of the acids in the form of air, nothing could be easier than to extend this process to the rest. I had nothing to do but either to procure the acid in a liquid form, viz. combined with water, and then expel the air by heat; or to find some solid substance in which it was combined, and, dislodging it by some stronger acid, to receive the generated air in quicksilver.

To procure the *vegetable acid air*, I was favoured by Dr. Higgins, with a quantity of exceedingly strong concentrated acid of vinegar, from which, by means of heat, and with the apparatus represented vol. I, plate 2. fig. 8. I could easily expel as much air, as from an equal quantity of spirit of salt. I found, however, that unless the apparatus was furnished with a small recipient, to intercept the liquor that might be thrown out of the vessel by boiling, I could not (except at the very first) procure this acid air free from moisture :

but with this provision I easily got the air as perfectly dry as I could wish.

This vegetable acid air extinguishes the flame of a candle, exactly like the vitriolic acid air, viz. without any particular colour of the flame in going out, or in lighting again.

Upon putting alkaline air to vegetable acid air, the white cloud observable in similar mixtures was instantly formed, and rose at once to the top of the vessel, as in the case of the other acid airs. The sides of the vessel in which this mixture was made, were tinged with yellow, as in the same process with vitriolic acid air; which to me is a puzzling fact, as I do not know that such a sulphur (if the substance be sulphur) was ever known to be formed without the vitriolic acid. At first I imagined that this colour had come from something contained in the ingredients for making the alkaline air; but I presently found, that when I put the alkaline air, from the very same preparation, to marine acid air, the salt formed by them was perfectly white, without the least tinge of yellow.

The affinities both of the marine and of the vitriolic acids in the form of air, have been
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seen to be considerably different from what they are when combined, as usual, with water; but in all the trials that I have made, the vegetable acid, even in this most advantageous form of air, appears to be weaker than any of the three mineral acids, exactly as might be concluded from what was known before concerning it. For this vegetable acid air was not able to decompose any substance into the composition of which any of the mineral acids entered. It made no impression upon *brimstone*, *salt-petre*, *common salt*, or *sal ammoniac*; nor yet upon *borax*.

Charcoal imbibes vegetable acid air very fast, and afterwards the smell of it is extremely pungent; but the air which remains seems not to have been altered by any thing that it had got from the charcoal.

Liver of sulphur imbibes vegetable acid air but slowly, and is neither discoloured nor dissolved by it. When only one tenth part of the air remained, I examined it, and found it to have nothing inflammable in it, which was the only effect that I had expected from it.

Water imbibes vegetable acid air as readily as any of the other acid airs. I once endeavoured to ascertain the quantity of this air that
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a given quantity of water would imbibe ; and to measure] the increase of weight and bulk that it might acquire by this impregnation, as I had in some measure done with respect to the marine acid, and alkaline airs : but the experiment did not succeed to my wish ; and I did not think it worth my while to attempt it again.

For this purpose I put a small quantity of water into a glass tube ; but it was no sooner introduced to the acid air, through the quicksilver, by which it was confined, than a small bubble of common air at the closed end of the tube began to swell, and it continued to do so till it threw out all the water. The case was the same when the end of the tube was hermetically sealed. I had the same result from *spirit of wine* introduced into this acid air, in the same circumstances, only the effect was produced much quicker. With *oil of turpentine* this effect was produced more quickly still ; but with *olive oil* much more slowly.

From this experiment I was led to imagine, that common air received a great expansion by the effluvium of this vegetable acid ; and I therefore expected that a quantity of the liquid acid admitted to common air, confined by quicksilver, would make it expand as
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ether had done ; but this was so far from being the case, that, after some time, the air appeared to be diminished, and extinguished a candle, so that it must have got phlogiston from the acid.

I made a second experiment of this kind, the result of which was, that a quantity of common air, which had been exposed six weeks to the effluvium of a small quantity of the liquid vegetable acid (contained in a cup, which swam upon the surface of the water, by which the air was confined) was so far injured, that two measures of it, and one of nitrous air did not occupy the space of quite two measures ; that is, it was almost as noxious as it could be made by any phlogistic process.

Suspecting that the water, which was rather foul, might have contributed to this injury, I exposed, for the last five weeks of the time, an equal quantity of common air, in a jar of the same size, standing in the same trough of water, and in all other respects in similar circumstances. But this air, though a little injured, was hardly to be distinguished from common air ; so that there could be no doubt but that, in the last-mentioned experiment,

the injury which the air had received, came from the effluvia of the vegetable acid.

Vegetable acid air is absorbed pretty readily by *olive oil*. A quantity of this oil absorbed about ten times its bulk of this air; and from being of a yellowish colour, as this oil naturally is, it became almost colourless, like water; which I thought not a little remarkable; as all the other acid airs deepen the colour of every species of oil, making them brown, and at the same time viscid, approaching to the consistence of resin; whereas this oil, in the experiment just now mentioned, became rather less viscid than before, a little approaching to the limpidity of water, or rather, more resembling an essential oil.

SECTION III.

Of DEPHLOGISTICATED Air, and of the constitution of the Atmosphere,

The contents of this section will furnish a very striking illustration of the truth of a remark, which I have more than once made in my philosophical writings, and which can hardly be too often repeated, as it tends greatly to encourage philosophical investigations; viz. that more is owing to what we call *chance*, that is, philosophically speaking, to the observation of *events arising from unknown causes*, than to any proper *design*, or pre-conceived *theory* in this business. This does not appear in the works of those who write *synthetically* upon these subjects; but would, I doubt not, appear very strikingly in those who are the most celebrated for their philosophical acumen, did they write *analytically* and ingenuously.

For my own part, I will frankly acknowledge, that, at the commencement of the experiments recited in this section, I was so far from having formed any hypothesis that led to the discoveries I made in pursuing them, that
they

they would have appeared very improbable to me had I been told of them ; and when the decisive facts did at length obtrude themselves upon my notice, it was very slowly, and with great hesitation, that I yielded to the evidence of my senses. And yet, when I re-consider the matter, and compare my last discoveries relating to the constitution of the atmosphere with the first, I see the closest and the easiest connexion in the world between them, so as to wonder that I should not have been led immediately from the one to the other. That this was not the case, I attribute to the force of prejudice, which, unknown to ourselves, biases not only our *judgments*, properly so called, but even the perceptions of our senses : for we may take a maxim so strongly for granted, that the plainest evidence of sense will not intirely change, and often hardly modify our persuasions ; and the more ingenious a man is, the more effectually he is entangled in his errors ; his ingenuity only helping him to deceive himself, by evading the force of truth.

There are, I believe, very few maxims in philosophy that have laid firmer hold upon the mind, than that air, meaning atmospherical air (free from various foreign matters, which were always supposed to be dissolved, and intermixed with it) is a *simple elementary substance*,
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indestructible, and unalterable, at least as much so as water is supposed to be. In the course of my inquiries, I was, however, soon satisfied that atmospherical air is not an unalterable thing; for that the phlogiston with which it becomes loaded from bodies burning in it, and animals breathing it, and various other chemical processes, so far alters and depraves it, as to render it altogether unfit for inflammation, respiration, and other purposes to which it is subservient; and I had discovered that agitation in water, the process of vegetation, and probably other natural processes, by taking out the superfluous phlogiston, restore it to its original purity. But I own I had no idea of the possibility of going any farther in this way, and thereby procuring air purer than the best common air. I might, indeed, have naturally imagined that such would be air that should contain less phlogiston than the air of the atmosphere; but I had no idea that such a composition was possible.

It will be seen in my last publication, that, from the experiments which I made on the marine acid air, I was led to conclude, that common air consisted of some acid (and I naturally inclined to the acid that I was then operating upon) and phlogiston; because the union of this acid vapour and phlogiston made
inflammable

inflammable air; and inflammable air, by agitation in water, ceases to be inflammable, and becomes respirable. And though I could never make it quite so good as common air, I thought it very probable that vegetation, in more favourable circumstances than any in which I could apply it, or some other natural process, might render it more pure.

Upon this, which no person can say was an improbable supposition, was founded my conjecture, of volcanos having given birth to the atmosphere of this planet, supplying it with a permanent air, first inflammable, then deprived of its inflammability by agitation in water, and farther purified by vegetation.

Several of the known phenomena of the *nitrous acid* might have led me to think, that this was more proper for the constitution of the atmosphere than the marine acid: but my thoughts had got into a different train, and nothing but a series of observations, which I shall now distinctly relate, compelled me to adopt another hypothesis, and brought me, in a way of which I had then no idea, to the solution of the great problem, which my reader will perceive I have had in view ever since my discovery that the atmospherical air is alterable, and therefore that it is not an elementary

mentary substance, but a *composition* viz. what this composition is, or *what is the thing that we breathe*, and how is it to be made from its constituent principles.

At the time of my former publication, I was not possessed of a *burning lens* of any considerable force ; and for want of one, I could not possibly make many of the experiments that I had projected, and which, in theory, appeared very promising. I had, indeed, a *mirror* of force sufficient for my purpose. But the nature of this instrument is such, that it cannot be applied, with effect, except upon substances that are capable of being suspended, or resting on a very slender support. It cannot be directed at all upon any substance in the form of *powder*, nor hardly upon any thing that requires to be put into a vessel of quicksilver ; which appears to me to be the most accurate method of extracting air from a great variety of substances, as was explained in the Introduction to this volume. But having afterwards procured a lens of twelve inches diameter, and twenty inches focal distance, I proceeded with great alacrity to examine, by the help of it, what kind of air a great variety of substances, natural and factitious, would yield, putting them into the vessels represented fig. *a*, which I filled with quick-

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silver,

silver, and kept inverted in a basin of the same. Mr. Warltire, a good chymist, and lecturer in natural philosophy, happening to be at that time in Calne, I explained my views to him, and was furnished by him with many substances, which I could not otherwise have procured.

With this apparatus, after a variety of other experiments, an account of which will be found in its proper place, on the 1st of August, 1774, I endeavoured to extract air from *mercurius calcinatus per se*; and I presently found that, by means of this lens, air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it, and found that it was not imbibed by it. But what surprized me more than I can well express, was, that a candle burned in this air with a remarkably vigorous flame, very much like that enlarged flame with which a candle burns in nitrous air, exposed to iron or liver of sulphur; but as I had got nothing like this remarkable appearance from any kind of air besides this particular modification of nitrous air, and I knew no nitrous acid was used in the preparation of *mercurius calcinatus*, I was utterly at a loss how to account for it.

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In this case, also, though I did not give sufficient attention to the circumstance at that time, the flame of the candle, besides being larger, burned with more splendor and heat than in that species of nitrous air; and a piece of red-hot wood sparkled in it, exactly like paper dipped in a solution of nitre, and it consumed very fast; an experiment which I had never thought of trying with nitrous air.

At the same time that I made the above-mentioned experiment, I extracted a quantity of air, with the very same property, from the common *red precipitate*, which being produced by a solution of mercury in spirit of nitre, made me conclude that this peculiar property, being similar to that of the modification of nitrous air above mentioned, depended upon something being communicated to it by the nitrous acid; and since the *mercurius calcinatus* is produced by exposing mercury to a certain degree of heat, where common air has access to it, I likewise concluded that this substance had collected something of *nitre*, in that state of heat, from the atmosphere.

This, however, appearing to me much more extraordinary than it ought to have done, I entertained some suspicion that the *mercurius calcinatus*, on which I had made my experiments,

being bought at a common apothecary's, might, in fact, be nothing more than red precipitate; though, had I been any thing of a practical chymist, I could not have entertained any such suspicion. However, mentioning this suspicion to Mr. Warltire, he furnished me with some that he had kept for a specimen of the preparation, and which, he told me, he could warrant to be genuine. This being treated in the same manner as the former, only by a longer continuance of heat, I extracted much more air from it than from the other.

This experiment might have satisfied any moderate sceptic; but, however, being at Paris in the October following, and knowing that there were several very eminent chymists in that place, I did not omit the opportunity, by means of my friend Mr. Magellan, to get an ounce of mercurius calcinatus prepared by Mr. Cadet, of the genuineness of which there could not possibly be any suspicion; and at the same time, I frequently mentioned my surprise at the kind of air which I had got from this preparation to Mr Lavoisier, Mr. le Roy, and several other philosophers, who honoured me with their notice in that city; and who, I dare say, cannot fail to recollect the circumstance.

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At the same time, I had no suspicion that the air which I had got from the mercurius calcinatus was even wholesome, so far was I from knowing what it was that I had really found ; taking it for granted, that it was nothing more than such kind of air as I had brought nitrous air to be by the processes above mentioned ; and in this air I have observed that a candle would burn sometimes quite naturally, and sometimes with a beautiful enlarged flame, and yet remain perfectly noxious.

At the same time that I had got the air above mentioned from mercurius calcinatus and the red precipitate, I had got the same kind from *red lead* or *minium*. In this process, that part of the minium on which the focus of the lens had fallen, turned yellow. One third of the air, in this experiment, was readily absorbed by water, but, in the remainder, a candle burned very strongly, and with a crackling noise.

That fixed air is contained in red lead I had observed before ; for I had expelled it by the heat of a candle, and had found it to be very pure. See Vol. I. p. 192. I imagine it requires more heat than I then used to expel any of the other kind of air.

This experiment with *red lead* confirmed me more in my suspicion, that the *mercurius calcinatus* must get the property of yielding this kind of air from the atmosphere, the process by which that preparation, and this of red lead is made, being similar. As I never make the least secret of any thing that I observe, I mentioned this experiment also, as well as those with the *mercurius calcinatus*, and the red precipitate, to all my philosophical acquaintance at Paris, and elsewhere; having no idea, at that time, to what these remarkable facts would lead.

Presently after my return from abroad, I went to work upon the *mercurius calcinatus*, which I had procured from Mr. Cadet; and, with a very moderate degree of heat, I got from about one fourth of an ounce of it, an ounce-measure of air, which I observed to be not readily imbibed, either by the substance itself from which it had been expelled (for I suffered them to continue a long time together before I transferred the air to any other place) or by water, in which I suffered this air to stand a considerable time before I made any experiment upon it.

In this air, as I had expected, a candle burned with a vivid flame; but what I observed

served new at this time, (Nov. 19,) and which surprized me no less than the fact I had discovered before, was, that, whereas a few moments agitation in water will deprive the modified nitrous air of its property of admitting a candle to burn in it; yet, after more than ten times as much agitation as would be sufficient to produce this alteration in the nitrous air, no sensible change was produced in this. A candle still burned in it with a strong flame; and it did not, in the least, diminish common air, which I have observed that nitrous air, in this state, in some measure, does.

But I was much more surprized, when, after two days, in which this air had continued in contact with water (by which it was diminished about one twentieth of its bulk) I agitated it violently in water about five minutes, and found that a candle still burned in it as well as in common air. The same degree of agitation would have made phlogisticated nitrous air fit for respiration indeed, but it would certainly have extinguished a candle.

These facts fully convinced me, that there must be a very material difference between the constitution of the air from mercurius calcinatus, and that of phlogisticated nitrous air,

notwithstanding their resemblance in some particulars. But though I did not doubt that the air from *mercurius calcinatus* was fit for respiration, after being agitated in water, as every kind of air without exception, on which I had tried the experiment, had been, I still did not suspect that it was respirable in the first instance; so far was I from having any idea of this air being, what it really was, much superior, in this respect, to the air of the atmosphere.

In this ignorance of the real nature of this kind of air, I continued from thistime (November) to the 1st of March following; having, in the mean time, been intent upon my experiments on the vitriolic acid air above recited, and the various modifications of air produced by spirit of nitre, an account of which will follow. But in the course of this month, I not only ascertained the nature of this kind of air, though very gradually, but was led by it to the complete discovery of the constitution of the air we breathe.

Till this 1st of March, 1775, I had so little suspicion of the air from *mercurius calcinatus*, &c. being wholesome, that I had not even thought of applying to it the test of nitrous air; but thinking (as my reader must imagine

gine I frequently must have done) on the candle burning in it after long agitation in water, it occurred to me at last to make the experiment; and putting one measure of nitrous air to two measures of this air, I found, not only that it was diminished, but that it was diminished quite as much as common air, and that the redress of the mixture was likewise equal to that of a similar mixture of nitrous and common air.

After this I had no doubt but that the air from mercurius calcinatus was fit for respiration, and that it had all the other properties of genuine common air. But I did not take notice of what I might have observed, if I had not been so fully possessed by the notion of there being no air better than common air, that the redness was really deeper, and the diminution something greater than common air would have admitted.

Moreover, this advance in the way of truth, in reality, threw me back into error, making me give up the hypothesis I had first formed, viz. that the mercurius calcinatus had extracted spirit of nitre from the air; for I now concluded, that all the constituent parts of the air were equally, and in their proper proportion, imbibed in the preparation of this substance,
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and also in the process of making red lead. For at the same time that I made the above-mentioned experiment on the air from mercurius calcinatus, I likewise observed that the air which I had extracted from red lead, after the fixed air was washed out of it, was of the same nature, being diminished by nitrous air like common air: but, at the same time, I was puzzled to find that air from the red precipitate was diminished in the same manner, though the process for making this substance is quite different from that of making the two others. But to this circumstance I happened not to give much attention.

I wish my reader be not quite tired with the frequent repetition of the word *surprize*, and others of similar import; but I must go on in that style a little longer. For the next day I was more surprized than ever I had been before, with finding that, after the above-mentioned mixture of nitrous air and the air from mercurius calcinatus, had stood all night, (in which time the whole diminution must have taken place; and, consequently, had it been common air, it must have been made perfectly noxious, and intirely unfit for respiration or inflammation) a candle burned in it, and even better than in common air.

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I cannot, at this distance of time, recollect what it was that I had in view in making this experiment; but I know I had no expectation of the real issue of it. Having acquired a considerable degree of readiness in making experiments of this kind, a very slight and evanescent motive would be sufficient to induce me to do it. If, however, I had not happened, for some other purpose, to have had a lighted candle before me, I should probably never have made the trial; and the whole train of my future experiments relating to this kind of air might have been prevented.

Still, however, having no conception of the real cause of this phenomenon, I considered it as something very extraordinary; but as a property that was peculiar to air extracted from these substances, and *adventitious*; and I always spoke of the air to my acquaintance as being substantially the same thing with common air. I particularly remember my telling Dr. Price, that I was myself perfectly satisfied of its being common air, as it appeared to be so by the test of nitrous air; though, for the satisfaction of others, I wanted a mouse to make the proof quite complete.

On the 8th of this month I procured a mouse, and put it into a glass vessel, contain-
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ing two ounce-measures of the air from mercurius calcinatus. Had it been common air, a full-grown mouse, as this was, would have lived in it about a quarter of an hour. In this air, however, my mouse lived a full half hour; and though it was taken out seemingly dead, it appeared to have been only exceedingly chilled; for, upon being held to the fire, it presently revived, and appeared not to have received any harm from the experiment.

By this I was confirmed in my conclusion, that the air extracted from mercurius calcinatus, &c. was, *at least, as good* as common air; but I did not certainly conclude that it was any *better*; because, though one mouse would live only a quarter of an hour in a given quantity of air, I knew it was not impossible but that another mouse might have lived in it half an hour; so little accuracy is there in this method of ascertaining the goodness of air: and indeed I have never had recourse to it for my own satisfaction, since the discovery of that most ready, accurate, and elegant test that nitrous air furnishes. But in this case I had a view to publishing the most generally-satisfactory account of my experiments that the nature of the thing would admit of.

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This experiment with the mouse, when I had reflected upon it some time, gave me so much suspicion that the air into which I had put it was better than common air, that I was induced, the day after, to apply the test of nitrous air to a small part of that very quantity of air which the mouse had breathed so long; so that, had it been common air, I was satisfied it must have been very nearly, if not altogether, as noxious as possible, so as not to be affected by nitrous air; when, to my surprise again, I found that though it had been breathed so long, it was still better than common air. For after mixing it with nitrous air, in the usual proportion of two to one, it was diminished in the proportion of $4\frac{1}{2}$ to $3\frac{1}{2}$; that is, the nitrous air had made it two ninths less than before, and this in a very short space of time; whereas I had never found that, in the longest time, any common air was reduced more than one fifth of its bulk by any proportion of nitrous air, nor more than one fourth by any phlogistic process whatever. Thinking of this extraordinary fact upon my pillow, the next morning I put another measure of nitrous air to the same mixture, and, to my utter astonishment, found that it was farther diminished to almost one half of its original quantity. I then put a third measure to it; but this did not diminish it any farther: but,

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however, left it one measure less than it was even after the mouse had been taken out of it.

Being now fully satisfied that this air, even after the mouse had breathed it half an hour, was much better than common air; and having a quantity of it still left, sufficient for the experiment, viz. an ounce-measure and a half, I put the mouse into it; when I observed that it seemed to feel no shock upon being put into it, evident signs of which would have been visible, if the air had not been very wholesome; but that it remained perfectly at its ease another full half hour, when I took it out quite lively and vigorous. Measuring the air the next day, I found it to be reduced from $1\frac{1}{2}$ to $\frac{2}{3}$ of an ounce-measure. And after this, if I remember well (for in my *register* of the day I only find it noted, that it was *considerably diminished* by nitrous air) it was nearly as good as common air. It was evident, indeed, from the mouse having been taken out quite vigorous, that the air could not have been rendered very noxious.

For my farther satisfaction I procured another mouse, and putting it into less than two ounce-measures of air extracted from mercurius calcinatus and air from red precipitate (which, having found them to be of the same quality,

quality, I had mixed together) it lived three quarters of an hour. But not having had the precaution to set the vessel in a warm place, I suspect that the mouse died of cold. However, as it had lived three times as long as it could probably have lived in the same quantity of common air, and I did not expect much accuracy from this kind of test, I did not think it necessary to make any more experiments with mice.

Being now fully satisfied of the superior goodness of this kind of air, I proceeded to measure that degree of purity, with as much accuracy as I could, by the test of nitrous air; and I began with putting one measure of nitrous air to two measures of this air, as if I had been examining common air; and now I observed that the diminution was evidently greater than common air would have suffered by the same treatment. A second measure of nitrous air reduced it to two thirds of its original quantity, and a third measure to one half. Suspecting that the diminution could not proceed much farther, I then added only half a measure of nitrous air, by which it was diminished still more; but not much, and another half measure made it more than half of its original quantity; so that, in this case, two measures of this air took more than two measures

tures of nitrous air, and yet remained less than half of what it was. Five measures brought it pretty exactly to its original dimensions.

At the same time, air from the *red precipitate* was diminished in the same proportion as that from *mercurius calcinatus*, five measures of nitrous air being received by two measures of this without any increase of dimensions. Now as common air takes about one half of its bulk of nitrous air, before it begins to receive any addition to its dimensions from more nitrous air, and this air took more than four half-measures before it ceased to be diminished by more nitrous air, and even five half-measures made no addition to its original dimensions, I conclude that it was between four and five times as good as common air. It will be seen that I have since procured air better than this, even between five and six times as good as the best common air that I have ever met with.

Being now fully satisfied with respect to the *nature* of this new species of air, viz. that, being capable of taking more phlogiston from nitrous air, it therefore originally contains less of this principle; my next inquiry was, by what means it comes to be so pure, or philosophically

sophically speaking, to be so much *dephlogisticated*; and since the red lead yields the same kind of air with *mercurius calcinatus*, though mixed with common air, and is a much cheaper material, I proceeded to examine all the preparations of lead, made by heat in the open air, to see what kind of air they would yield, beginning with the *grey calx*, and ending with *litharge*.

The red lead which I used for this purpose yielded a considerable quantity of dephlogisticated air, and very little fixed air; but to what circumstance in the preparation of this lead, or in the keeping of it, this difference is owing, I cannot tell. I have frequently found a very remarkable difference between different specimens of red lead in this respect, as well as in the purity of the air which they contain. This difference, however, may arise in a great measure, from the care that is taken to extract the fixed air from it. In this experiment two measures of nitrous air being put to one measure of this air, reduced it to one third of what it was at first, and nearly three times its bulk of nitrous air made very little addition to its original dimensions; so that this air was exceedingly pure, and better than any that I had procured before.

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The preparation called *massicot* (which is said to be a state between the grey calx and the red lead) also yielded a considerable quantity of air, of which about one half was fixed air, and the remainder was such, that when an equal quantity of nitrous air was put to it, it was something less than at first; so that this air was about twice as pure as common air.

I thought it something remarkable, that in the preparations of lead by heat, those before and after these two, viz. the red lead and *massicot*, yielded only fixed air. I would also observe, by the way, that a very small quantity of air was extracted from *lead ore* by the burning lens. The bulk of it was easily absorbed by water. The remainder was not affected by nitrous air, and it extinguished a candle.

I got a very little air by the same process from the *grey calx of lead*, of precisely the same quality with the former. That part of it which was not affected by nitrous air extinguished a candle, so that both of them may be said to have yielded fixed air, only with a larger portion than usual, of that part of it which does not unite with water.

Litharge

Litharge (which is a state that succeeds the red lead) yielded air pretty readily; but this also was fixed air. That which was not absorbed by water, was not affected by nitrous air.

Much more than I had any opportunity of doing remains to be done, in order to ascertain upon what circumstances, in these preparations of lead, the quality of the air which they contain, depends. It can only be done by some person who shall carefully attend to the processes, so as to see himself in what manner they are made, and examine them in all their different states. I very much wished to have attempted something of this kind myself, but I found it impossible in my situation. However, I got Dr. Higgins (who furnished me with several preparations that I could not easily have procured elsewhere) to make me a quantity of red lead, that I might, at least, try it when *fresh made*, and after keeping it some time in different circumstances; and though, by the help of this preparation, I did not do the thing that I expected, I did something else, much more considerable.

This fresh made red lead had a yellowish cast, and had in it several pieces intirely yellow. I tried it immediately, in the same man-

ner in which I had made the preceding experiments, viz. with the burning lens in quick-silver, and found that it yielded very little air, and with great difficulty; requiring the application of a very intense heat. With an equal quantity of nitrous air, a part of this air was reduced to one half of its original bulk, and $3\frac{1}{2}$ measures saturated it. The air, therefore, was very pure, and the quantity that it yielded being very small, it proved to be in a very favourable state for ascertaining on what circumstances its acquiring this air depended.

My object now was to bring this fresh made red lead, which yielded very little air, to that state in which other red lead had yielded a considerable quantity; and taking it, in a manner, for granted, in consequence of the reasoning intimated above, that red lead must imbibe from the atmosphere some kind of acid, in order to acquire that property, I took three separate half-ounces of this fresh made red lead, and moistened them till they made a kind of paste, with each of the three mineral acids, viz. the vitriolic, the marine, and the nitrous; and as I intended to make the experiments in a gun-barrel, lest the iron should be too much affected by them, I dried all these mixtures, till they were perfectly hard; then
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pulverizing them, I put them separately into my gun-barrel, filled up to the mouth with pounded flint, which I had found by trial to yield little, or no air when treated in this manner. I had also found that no quantity of air, sufficient to make an experiment, could be procured from an equal quantity of this red lead by this process.

Those portions of the red lead which had been moistened with the vitriolic and marine acids became white; but that which had been moistened with the nitrous acid, had acquired a deep brown colour. The mixtures with the nitrous and marine acids dried pretty readily, but that with the vitriolic acid was never perfectly dry; but a great part of it remained in the form of a softish paste.

Neither the vitriolic nor the marine acid mixtures gave the least air when treated in the manner above mentioned; but the moment that the composition into which the *nitrous* acid had entered became warm, air began to be produced; and I received the produce in quicksilver. About one ounce-measure was quite transparent, but presently after it became exceedingly red; and being satisfied that this redness was owing to the nitrous acid vapour having dissolved the quicksilver, I took

no more than two ounce-measures in this way, but received all the remainder, which was almost two pints, in water. Far the greatest part of this was fixed air, being readily absorbed by water, and extinguishing a candle. There was, however, a considerable residuum, in which the flame of a candle burned with a crackling noise, from which I concluded that it was true dephlogistified air.

In this experiment I had moistened the red lead with spirit of nitre several times, and had dried it again. When I repeated the experiment, I moistened it only once with the same acid, when I got from it not quite a pint of air; but it was almost all of the dephlogistified kind, about five times as pure as common air. N. B. All the acids made a violent effervescence with the red lead.

Though there was a difference in the result of these experiments, which I shall consider hereafter, I was now convinced that it was the nitrous acid which the red lead had acquired from the air, and which had enabled it to yield the dephlogistified air, agreeable to my original conjecture. Finding also, as will be seen in the following section, that the same kind of air is produced by moistening with the spirit of nitre any kind of earth that is free from
phlogiston,

phlogiston, and treating it as I had done the red lead in the last-mentioned experiment, there remained no doubt in my mind, but that *atmospherical air*, or the thing that we breathe, *consists of the nitrous acid and earth*, with so much phlogiston as is necessary to its elasticity; and likewise so much more as is required to bring it from its state of perfect purity to the mean condition in which we find it.

For this purpose I tried, with success, *flowers of zinc, chalk, quick-lime, slacked-lime, tobacco-pipe clay, flint and Muscovy talck*, with other similar substances, which will be found to comprize all the kinds of earth that are essentially distinct from each other, according to their chymical properties. A particular account of the processes with these substances, I reserve for another section; thinking it sufficient in this to give a history of the discovery, and a general account of the nature of this dephlogisticated air, with this general inference from the experiment, respecting the constitution of the atmosphere.

I was the more confirmed in my idea of spirit of nitre and earth constituting respirable air, by finding, that when any of these matters, on which I had tried the experiment, had been treated in the manner above mentioned,

and they had thereby yielded all the air that could be extracted from them by this process; yet when they had been moistened with fresh spirit of nitre, and were treated in the same manner as before, they would yield as much dephlogisticated air as at the first. This may be repeated till all the earthy matter be exhausted. It will be sufficient to recite one or two facts of this kind from my register.

April 18, I took the remains of the fresh made red lead, out of which a great quantity of dephlogisticated air had been extracted, and moistening about three quarters of an ounce of it a second time with spirit of nitre, I got from it about two pints of air, all of which was nearly six times as pure as common air. This air was generated very fast, and the glass tube through which it was transmitted was filled with red fumes; the nitrous acid, I suppose, prevailing in the composition of the air, but being absorbed by the water in which it was afterwards received.

In this, and many other processes, my reader will find a great variety in the purity of the air procured from the same substances. But this will not be wondered at, if it be considered that a small quantity of phlogistic matter, accidentally mixing with the ingredients for the composition

composition of this air, depraves it. It will also be unavoidably depraved, in some measure, if the experiment be made in a gun-barrel, which I commonly made use of, when, as was generally the case, it was sufficiently exact for my purpose, on account of its being the easiest, and in many respects, the most commodious process.

The reason of this is, that if the produce of air be not very rapid, there will be time for the phlogiston to be disengaged from the iron itself, and to mix with the air. Accordingly I have seldom failed to find, that when I endeavoured to get all the air I possibly could from any quantity of materials, and received the produce at different times (as for my satisfaction I generally did) the last was inferior in purity to that which came first. Not unfrequently it was phlogisticated air; that is, air so charged with phlogiston, as to be perfectly noxious; and sometimes, as the reader will find in the next section, it was even nitrous air.

On the same account it frequently happened, that when I used a considerable degree of heat, the red lead which I used in these experiments would be changed into real lead, from
which

which it was often very difficult to get the gun-barrel perfectly clear.

A good deal will also depend upon the ingredients which have been used in the gun-barrel in preceding experiments: for it is not easy to get such an instrument perfectly clean from all the matters that have been put into it: and though it may be presumed, in general, that every kind of air will be expelled from such ingredients by making the tube red-hot; yet matters containing much phlogiston, as charcoal, &c. will not part with it in consequence of the application of heat, unless there be at hand some other substance with which it may combine. Though, therefore, a gun-barrel, containing such small pieces of charcoal as cannot be easily wiped out of it, be kept a long time in a red heat, and even with its mouth open; yet if it be of a considerable length, some part of the charcoal may remain unconsumed, and the effect of it will be found in the subsequent experiment. Of this I had the following very satisfactory proof.

Being desirous to shew some of my friends the actual production of dephlogistified air, and having no other apparatus at hand; I had recourse to my gun-barrel; but apprized them, that having used it the day before to get air
from

from charcoal, with which it had been filled for that purpose, though I had taken all the pains I could to get it all out, yet so much would probably remain, that I could not depend on the air I should get from it being dephlogisticated; but that it would probably be of an inferior quality, and perhaps even nitrous air. Accordingly, having put into it a mixture of spirit of nitre and red lead (being part of a quantity which I had often used before for the same purpose) dried, and pounded, I put it into the fire, and received the air in water.

The first produce, which was about a pint, was so far nitrous, that two measures of common air, and one of this, occupied the space of little more than two measures; that is, it was almost as strongly nitrous as that which is produced by the solution of metals in spirit of nitre. The second pint was very little different from common air, and the last produce was better still, being more than twice as good as common air. If, therefore, any person shall propose to make dephlogisticated air, in large quantities, he should have an apparatus appropriated to that purpose; and the greatest care should be taken to keep the instruments as clear as possible from all phlogistic matter, which is the very bane of purity

rity with respect to air, they being exactly *plus* and *minus* to each other.

The hypothesis maintained in this section, viz. that atmospherical air consists of the nitrous acid and earth, suits exceedingly well with the facts relating to the production of nitre; for it is never generated but in the open air, and by exposing to it such kinds of earth as are known to have an affinity with the nitrous acid; so that by their union common nitre may be formed.

Hitherto it has been supposed by chymists, that this nitrous acid, by which common nitre is formed, exists in the atmosphere as an *extraneous substance*, like water, and a variety of other substances, which float in it, in the form of effluvia; but since there is no place in which nitre may not be made, it may, I think, with more probability be supposed, according to my hypothesis, that nitre is formed by a real *decomposition of the air itself*, the *bases* that are presented to it having, in such circumstances, a nearer affinity with the spirit of nitre than that kind of earth with which it is united in the atmosphere.

My theory also supplies an easy solution of what has always been a great difficulty with chymists, with respect to the *detonation of nitre*.

nitre. The question is, what becomes of the nitrous acid in this case? The general, I believe the universal, opinion now is, that it is *destroyed*; that is, that the acid is properly decomposed, and resolved into its original elements, which Stahl supposed to be earth and water. On the other hand, I suppose that, though the common properties of the acid, as combined with water, disappear, it is only in consequence of its combination with some earthy or inflammable matter, with which it forms some of the many species of air, into the composition of which this wonderful acid enters. It may be common air, it may be dephlogisticated air, or it may be nitrous air, or some of the other kinds, of which an account will be given in a subsequent section. That it should really be the nitrous acid, though so much disguised by its union with earthy, or other matters, will not appear extraordinary to any person who shall consider how little the acid of vitriol is apparent in common sulphur.

With respect to *mercurius calcinatus*, and *red lead*, their red colour favours the supposition of their having extracted spirit of nitre from the air.

SECTION IV.

A more particular Account of some Processes for the Production of Dephlogisticated Air.

I cannot promise those of my readers, whose object is nothing more than *general information*, much pleasure from the perusal of this section, as it will consist, for the most part, of a dry detail of processes, for procuring dephlogisticated air; but as they all appeared necessary, in my investigation of the subject, I doubt not but an attention to them will be of use to such as are disposed to pursue the inquiry themselves. I might have contented myself with giving a general idea of the result of such experiments; but that would have been to mix my own *opinions* with *facts*, in such a manner that the reader would not have been able to separate them. At present, if I should be mistaken in any of my opinions, the reader, having before him all the facts on which those opinions were grounded, will be able to rectify the mistake, and prevent the error from spreading.

Having seen sufficient reason to conclude that respirable air consists of nitrous acid and earth, my object, in all this course of experiments,

ments, was simply to find *what kind of earth* was most proper for this purpose, or which had the most aptness to form this peculiar union with the nitrous acid. Upon the whole, I think it will appear that the *metallic earths*, if they be free from phlogiston, are the most proper, and next to them the *calcareous earths*; but that a very great difference in the production of this kind of air depends upon a variety of circumstances in which the experiments are made.

I have observed that *red lead*, without any addition, yields dephlogisticated air by heat. To give some idea of the differences in the results, from what is, to appearance, the same preparation, and of the consequence of adding spirit of nitre to the red lead, I must inform my reader, that having weighed two half-ounces of red lead, taken from the same parcel, I put one of them, without any addition, into the gun-barrel, and with a very brisk fire (which is generally a considerable advantage for the production of air) I got no more than three ounce-measures, and it was very little better than common air.

The second half-ounce I moistened with a very diluted spirit of nitre; and when it was dried and pounded, I put it into the same gun-barrel;

barrel; and, in the same circumstances with the former, I got from it about three pints of air, the first part of which was so far dephlogisticated, that two measures of it, and five of nitrous air, occupied the space of two measures only; of the second quantity, two measures were not increased by the addition of seven measures of nitrous air. This was the purest air that I had then seen. The last produce was almost all pure fixed air, being not at all affected by nitrous air. extinguishing a candle, and precipitating lime in lime-water. It was, indeed, a little of a nitrous nature; for it diminished common air in a small degree, an effect which I attribute to the phlogiston coming from the iron.

A remarkable difference in the quantity of the produce of this kind of air, as I hinted just now (and as I have observed in a former publication, in the produce of inflammable air) depends upon the *suddenness* with which the same degree of heat is applied. The following must be reckoned a remarkable fact of this kind, and it was made with as much care as I could possibly apply. From an ounce of red lead, by a sudden and brisk heat, I got above two quarts of air, a great part of which was fixed air, and the rest was about twice as good as common air; and immediately after,
 putting

putting the very same quantity of the same parcel of red lead into the same gun-barrel, by heat very *slowly applied*, but urged vehemently at last, I got no more than two ounce-measures of air, a great part of which was fixed air, and the rest not so good as common air.

I had been told that red lead acquired additional weight by being often washed in water. In order to try whether this was the fact, and also whether the red lead acquired its property of yielding dephlogisticated air by this means, I washed a quantity of that parcel which I had got fresh-made, four times in distilled water, evaporating it to dryness each time; but no more air came from it than when it had not been wetted, neither was it at all increased in weight by this means.

I have observed that, in general, those substances which, without containing phlogiston, yield fixed air with heat, or by the addition of an acid, when mixed with spirit of nitre, and treated as above, yield more or less of dephlogisticated air; but generally with a considerable mixture of fixed air, though I profess not to know upon what circumstance it is that the proportion of these two kinds of air, produced from these substances, depends,

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With a very small degree of heat, *white lead*, without any addition, yields a very great quantity of pure fixed air. Having moistened about an ounce-measure of it with spirit of nitre, and having put it into a glass-vessel, with a ground-stopple and tube, I extracted from it, at five different times, five pints of air, each of which I examined separately, as usual, and the results were as follows.

Of the first quantity, about $\frac{1}{2} \frac{9}{10}$ or $\frac{2}{3} \frac{9}{10}$ was absorbed by water, and the remainder neither affected common air, nor was affected by nitrous air, so that it was pure fixed air; and considering the quantity that would be necessarily absorbed by the water in which it was received, before I made any trial of the properties of it, it may perhaps be deemed to have been as free from any foreign mixture as any that was ever procured. Of the second quantity, about twice as much was left unabsorbed by water, and the residuum appeared to be dephlogisticated; for it took about an equal measure of nitrous air to saturate it; and consequently it was nearly twice as good as common air. Of the third quantity, as little remained unabsorbed by water as of the first; but the residuum was as pure as that of the second quantity. Of the fourth quantity, one-fourth remained unabsorbed by water, and it took

took $1 \frac{3}{4}$ of nitrous air to saturate it. Of the fifth pint, one-half remained unabsoꛑbed, and it took more than two equal measures of nitrous air to saturate it; so that it was nearly four times as pure as common air. Lastly, a single ounce-measure, that came very slowly after the five pints, was no part of it absoꛑbed by water, and it took $1 \frac{1}{2}$ of nitrous air to saturate it, being about three times as pure as common air.

From a quantity of *litharge*, moistened with spirit of nitre, and dried, I got, in a gun-barrel, a great quantity of air; about half of which was fixed air, precipitating lime in lime-water, and the other half was strongly nitrous; but with a burning lens, in quicksilver, I got a very pure dephlogisticated air from this mixture.

To go through the different states of lead in this manner, I took half an ounce of *lead-ore*, and having saturated it with spirit of nitre, I dried it as before; put it into a gun-barrel, filled up to the mouth with pounded flint, and placed vessels filled with water to receive the air. The consequence was, that as soon as this mixture began to be warm, air was generated very fast, insomuch that, being rather alarmed, I stood on one side; when presently

there was a violent and loud explosion, by which all the contents of the gun-barrel were driven out with great force, dashing to pieces the vessels that were placed to receive the air, and dispersing the fragments all over the room; so that all the air which I had collected, and which was about a pint, was lost. The mixture, before it was put into the gun-barrel, was betwixt white and yellow, and had very much the smell of brimstone; so that it was in fact a composition similar to gun-powder.

Being desirous to know what kind of air I had got by this process, I put the same materials into a glass-phial, and putting it into a crucible with sand, disposed the apparatus for receiving the air in such a manner, that the explosion could not affect it. It did explode as before, but the air was preserved, and appeared to be very strong nitrous air, almost as much so as that which is procured by the solution of metals.

From the *grey calx of lead*, treated in the same manner, I got about a pint of air, half of which, being readily absorbed by water, I take for granted, was fixed air; but the remainder was strongly nitrous. Had I not washed it a good deal in water, it would probably

bably have been as strong as that which is procured from metals.

The purest air that I ever procured was from *flowers of zinc*, moistened, as in the other processes, with spirit of nitre, and put into a glass-phial, with a ground-stopple and tube. At first I despaired of getting any air at all from the process; but at length it came in a prodigious torrent, and was so cloudy, that the bursting of every bubble, after it had passed through the water, resembled the bursting of a bag of flour. The tube through which it was transmitted was exceedingly red, and in some degree, the inside of the receiver too, as might be perceived amidst the thick cloud that filled it. This cloudiness of the newly-generated air, I have often perceived in the process with red-lead, but never in so great a degree as in this case.

The quantity of air procured was nearly three pints, from about half an ounce-measure of the flowers of zinc; and it was so highly dephlogisticated, that it took three times its bulk of nitrous air before its dimensions were increased. When it had got only twice its bulk of nitrous air, it was reduced to less than one-fifth of its original quantity. The last produce came very slowly, and was not quite

so pure. The flowers of zinc, which I used in this experiment, I had from Dr. Higgins. They formed a very hard and brittle substance, when mixed with spirit of nitre, and dried. After the process it swelled, and broke the phial into many pieces.

Besides these, I tried no earth of any metal except the *rust of iron* and *white arsenic*, both of which, when treated in the manner above mentioned, and put into a gun-barrel, yielded nothing but fixed or nitrous air; so that these calces undoubtedly contained much phlogiston, and the flowers of zinc, perhaps, none at all.

From considerably less than half an ounce of *rust of iron*, moistened with spirit of nitre, and dried, I got about a quart of air, about one-third of which was fixed air, precipitating lime in lime-water, &c. and the remainder was nitrous; so that two measures of common air and one of this, occupied the space of less than two measures.

The *white arsenic* I procured from Dr. Higgins, who assured me that it contained the least phlogiston possible. I moistened about an ounce-measure of it with spirit of nitre, and putting it into a phial with a ground-stopper and tube, with no great degree of heat,
I extracted

I extracted from it four ounce-measures of air, and it was as strongly nitrous as any that I had ever procured from metals. I increased the heat till the phial was melted, without getting any more air. The tube was exceedingly red during the transmission of the air through it.

Next, to the metallic earths of lead and zinc, I found the *calcareous* earths the most proper for the production of dephlogisticated air; but I had no opportunity of trying any great variety of them. The best that I did try was *chalk*. Having saturated half an ounce of it with diluted spirit of nitre, and dried it, I got from it, in a gun-barrel, more than a pint of air, which was highly dephlogisticated. I began to receive this produce in quicksilver, the consequence of which was, that the nitrous acid, coming over in the form of vapour, dissolved the quicksilver, and made nitrous air; but a crust being formed upon the surface of it, prevented the solution of more, and the air continued red a long time.

From another ounce-measure of chalk, treated in the same manner, I got about a quart of air. What I took first was considerably nitrous, two measures of common air and one of this, occupying the space of $2\frac{1}{2}$ measures: The second pint was dephlogisticated; so that

two measures of it, and five of nitrous air, occupied the space of two measures. The last was less dephlogisticated, being about one-half better than common air. At this time the air was generated with prodigious rapidity; the glass-tube through which it was transmitted was exceedingly red; and when, in changing the vessels, some of the vapour escaped into the air, it had the reddest appearance of any thing that I had ever seen of the kind.

Having saturated half an ounce of exceedingly good *quick-lime* with diluted spirit of nitre, dried it, and put it into a gun-barrel, I got from it about a pint and half of air, the first part of which was so far dephlogisticated, that it required an equal measure of nitrous air to saturate it. The second was no better than common air, and the third was equal to the first. In this process the air was produced very irregularly, sometimes coming in great quantities, and at other times the water would rush back into the tube.

I repeated the experiment on quick-lime, in a glass-phial and tube, when the whole quantity was so pure, that it required twice its bulk of nitrous air to saturate it. The produce of air, in this experiment, was as irregular as in the
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the preceding. I could have wished to have treated the stone from which that lime was made in the same manner, but I had no opportunity.

From *lime fallen in air*, moistened with spirit of nitre, and treated as above, in a gun-barrel, I got near a pint of air, the greatest part of which came very rapidly, the fire being urged very much; and it was so far dephlogisticated, that two measures of it required five measures of nitrous air to saturate it. The last produce came very slowly, and it was so far nitrous, that two measures of common air, and one of it, occupied the space of less than two measures; that is, it was very nearly perfectly nitrous.

I also moistened with spirit of nitre a quantity of *lime that had been plunged in water*, in order to make lime-water, and got air from it in a gun-barrel, very irregularly, as before: one part of this air, which came almost at once, was dephlogisticated, so that two measures of this, and five of nitrous air occupied the space of $2\frac{1}{2}$ measures.

From two ounce-measures of pounded *marble*, treated as above, in a gun-barrel, I got about three quarts of air; but a very great proportion

tion of it was fixed air, especially the last produce, which indeed was very little else; but towards the beginning of the process, the residuum was a little better than common air.

Repeating this experiment on marble, in a glass-phial with a ground-stopper and tube, I extracted from an ounce-measure of it about two pints of air, the greatest part of which was so highly dephlogisticated, that it took nearly three equal quantities of nitrous air to saturate it. Even the last produce was hardly to be distinguished from the first. What remained in the phial after the experiment swelled and broke it.

From *magnesia*, both calcined and uncalcined, I got, in a gun-barrel, a considerable quantity of air. From the calcined *magnesia* it was not much better than common air; from the uncalcined it was more than twice as good. But very probably this difference may not be invariable.

I think it very probable that dephlogisticated air may be procured from any kind of earth with which the spirit of nitre will unite; especially if it will likewise admit of a combination with fixed air or alkaline air, so that the spirit of nitre must expel the fixed air or
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alkaline air, before it can incorporate with it. Of substances of these kinds, besides those above mentioned, I tried *salt of tartar* and *wood-ashes*.

From half an ounce of *salt of tartar*, moistened with smoking spirit of nitre, and dried, I got, in a gun-barrel, about half a pint of air, the greatest part of which was fixed air, with a residuum so far dephlogisticated, as to be about three times as good as common air. The produce of air, in this experiment, was not very rapid, and it continued a long time. More would have been collected; but that part of it escaped at the luting.

I moistened about half an ounce-measure of *ashes of wood*, carefully burned, first in an iron-ladle, and then in a crucible, with strong smoking spirit of nitre; and, in a gun-barrel, I got from it about three pints of air, part of which was fixed, precipitating lime in lime-water, &c. and the rest was so pure, that it absorbed nearly three times its bulk of nitrous air. The last produce was very slow, and only about twice as good as common air.

From an ounce-measure of ashes of *pit-coal*, burned with all possible care, and treated in the same manner as above, I got about three
quarts

quarts of air, one-third of which was fixed air, precipitating lime in lime-water; but the residuum was strongly nitrous, especially at the last. It is observable enough, from their dark colour (of which no burning, I believe, will divest them) that, in general, ashes of pit-coal contain much more phlogiston than ashes of wood.

N. B. In this, and other processes, it will be observed, that fixed air was procured from substances, which can hardly be thought to have contained it before: which affords a presumption, that that it is not an acid *sui generis*, but a modification of the nitrous acid.

Clay is a substance altogether different from calcareous earth, and is not supposed, I believe, to contain any air at all. Of that species of it, which is called *tobacco-pipe-clay*, and which, I believe, is the purest of all, I got from Dr. Higgins, a quantity in powder; and moistening it with spirit of nitre, I observed that no more heat or effervescence was produced than the mixing of it with water would have occasioned.

Putting it, when dry, into a gun-barrel, I extracted from it, by a strong heat, about two ounce-measures of air, which being pretty readily

dily absorbed by water, neither affecting common air, nor being affected by nitrous air, and extinguishing a candle, I concluded to be fixed air. Repeating the experiment, I got the same produce, only observing, that the fixed air made lime-water turbid, the most certain test, I believe, of the presence of fixed air; and the last produce was highly nitrous. Imagining that this produce might have come from the phlogiston of the iron, I resolved to repeat the experiment once more, with all possible care, in a glass-phial, with a ground-stopper and tube.

I did so, and took the produce at eight different times. The first and second quantities had a good deal of fixed air in them; the residuum of the first was a little diminished by nitrous air, almost as much as air in which a candle had burned out, which might be owing, in part, to the common air contained in the phial. The residuum of the second quantity, on the other hand, diminished common air a little; so that two measures of common air, and one of this, occupied the space of $2\frac{1}{3}$ measures. Of the third quantity, two measures required three measures of nitrous air to saturate it; so that it was pretty highly dephlogisticated. Of the fourth, two measures,
and

and three of nitrous air, occupied the space of $1 \frac{3}{4}$ measures. The fifth was of the same quality with the third. The sixth required twice its quantity of nitrous air to saturate it. The seventh was not quite so pure as the sixth; and the eighth neither affected common air, nor was affected by nitrous air, being what I term *phlogisticated air*. As some part of this produce was nitrous air, it is evident that the phlogiston necessary to constitute it must have been in the clay, and not in the vessel containing it, which was of glass.

Having by me a quantity of *Stourbridge clay*, I had the curiosity to repeat the experiments with this species, to see whether there would be any material difference in the result. Using the gun-barrel, I received the air in four portions. The first was fixed air, making lime-water turbid, and being more than half-absorbed by water; the second was about as good as common air, and the fourth was considerably nitrous.

To avoid the effect of the gun-barrel, I then put the clay into a phial, with a glass-stopper and tube, and putting it into a sand-heat, I received the air, for the sake of greater exactness, in ten different portions, about one-half of an ounce-measure each. The first produce
was

was half-absorbed by water, with a residuum so far nitrous, that two measures of common air and one of this, occupied the space of $2\frac{1}{2}$ measures. The second and third portions were almost wholly fixed air, precipitating lime in lime-water, and not at all affecting common air, or being affected by nitrous air. Of the fourth I have no account. The fifth was so far dephlogisticated, that two measures of it, and three of nitrous air, occupied the space of $2\frac{1}{2}$ measures. The sixth and seventh produce were, as nearly as possible, common air. The ninth was so far nitrous, that two measures of common air, and one of this, occupied the space of $2\frac{1}{2}$ measures; and the tenth diminished common air still less.

It is evident, from the course of this process, that phlogiston must have been contained in the clay, and have been disengaged at different times, according as the heat affected different parts of the mixture. Had the whole of this produce been taken together, it would have been about the standard of common air mixed with fixed air; which shews the importance of taking the produce in different vessels, and examining them separately; a practice which the reader will find I have often had recourse to with great advantage.

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As a great degree of heat cannot be applied to any thing contained in a glass-vessel, without melting it, and I was willing to know what would be the effect of more heat on this very clay when the above mentioned experiment was over, I took it out of the phial, and put it into a gun-barrel; when I got a considerable quantity of air from it. Part of it was fixed air, precipitating lime in lime-water, and the remainder was like the residuum of fixed air, or phlogisticated common air, extinguishing a candle, and being neither affected by nitrous air, nor affecting common air. I did the same thing with the *tobacco-pipe-clay*, which remained after the experiment above recited, and had nearly the same result. The first produce was of the same degree of purity with common air, and the next was a little affected by nitrous air.

From a quantity of *gypsum*, which I procured in the form of powder, I got a quantity of fixed air in a gun-barrel; and from the same, moistened with spirit of nitre, and treated in the same manner, I got a little fixed air, with a great proportion of nitrous air, almost as strong as any. But suspecting that this gypsum was not pure, I got of Dr. Higgins a piece of that kind of which the finest plaster is made, and from this, mixed with spirit of nitre, I got
a considerable

a considerable quantity of air, part of which was fixed air, and the remainder neither affected common air, nor was affected by nitrous air, and extinguished a candle. At last the air was nitrous, as I suspect, from the gun-barrel.

Being rather surprized that this kind of earth, which had the appearance of being very free from phlogiston, should yield air of no better quality than this, I repeated the experiment, by taking the produce of air at several times, as in former experiments, moistening the earth with a stronger spirit of nitre than before; and instead of a gun-barrel, made use of a phial with a ground-stopple and tube. The quantity of air produced in this manner, was about two ounce-measures, from an ounce-measure of the plaister, and I received the air in four different parts.

The first was a little diminished by nitrous air, being, I suppose, in a great measure, common air not quite expelled from the phial; and the second was strong nitrous air, perhaps from some phlogistic matter accidentally mixed with the ingredients. I am the more induced to think so, because the third and fourth produce was so highly dephlogisticated, that one measure of each took five measures of nitrous air

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to faturate it; so that they were each four times as good as common air.

After the preceding experiments, there remained only the *crystalline* and *talcky earths*, that are essentially different from each other; and each of these also yielded dephlogisticated air, when they were treated in the same manner with the earths above mentioned, but in a very small quantity.

When I took common *flints*, as they are dug out of the ground, part white and part black, moistening the powder of them with spirit of nitre, as before, and using a gun-barrel, I got fixed air, with a great proportion of nitrous air; that which came over the first being like the residuum of fixed air, extinguishing a candle, but being not readily absorbed by water.

After this I got some *flints carefully calcined in close vessels*, by Dr. Higgins, and having pounded a quantity of them, and moistened the powder with spirit of nitre, I put it into a glass-phial with a ground stopple and tube; and applying, at first, the flame of a candle only, the air I got was in a very small quantity; but it precipitated lime in lime-water, and diminished common air a little.

I then put the same apparatus into a sand-heat, when I got, in all, as much air as twice the bulk of the materials. Part of it precipitated lime in lime-water, but the rest of the produce was considerably better than common air; and the last was so good, that it took two measures of nitrous air to saturate it.

Left this air might come from some extraneous matter, mixed with the powder of flint, I put some fresh spirit of nitre upon the same materials, without taking them out of the phial, after I had found that they would yield no more air from the first process, and I replaced the phial in the same sand-heat. The air first produced in this second process was but little diminished by nitrous air, but the rest was almost as pure as any that I had ever got before. The quantity of it, however, was not more than the bulk of the materials.

N. B. When, in this experiment, the bubbles of air burst, after getting through the water, a whitish cloud issued from them, as in the rapid production of nitrous air, and as in the produce of dephlogisticated air from the flowers of zinc above mentioned, but in a much less degree.

I repeated the same process not less than half a dozen times, putting fresh spirit of nitre upon the same materials, without taking them out of the phial, but the result was always the same; the first produce of air being always phlogisticated, then (after an interval in which nothing but the pure vapour of spirit of nitre came over) the remainder being the dephlogisticated air above mentioned.

To complete this course of experiments, I, in the last place, put strong spirit of nitre into a phial, filled with transparent *Muscovy talc*, such as opticians make use of for confining microscopic objects. In this process every thing went on in the very same manner as with the calcined flint; the first produce being phlogisticated air, or air of such quality as neither to affect common air, nor be affected by nitrous air, then the pure vapour of the spirit of nitre; and lastly, about an ounce-measure of air, about five times as good as common air. The pieces of talc which had been contiguous to the sides of the phial appeared to be a little whitened after the experiment, but the rest looked as if they had never been used in that manner; being as transparent as before, and of as firm a texture, but seemingly more flexible; so that those pieces, when handled all together, felt like soft feathers.

It is sufficiently evident from these experiments, that dephlogistified air is produced from all kinds of earth mixed with spirit of nitre, only that a greater quantity of air is produced from some than from others; the advantage in this respect being on the side of the metallic and calcareous earths.

I would observe, that this process seems to furnish a pretty accurate test, perhaps the most accurate hitherto known, of the presence of phlogiston in bodies. Perhaps no species of air can be produced without a certain portion of phlogiston; but probably the nitrous acid itself always contains sufficient for the purpose of dephlogistified air. But nitrous air contains so much phlogiston, that I think it cannot be produced unless the materials themselves contain it in a very considerable degree. Thus I have no doubt but that *white arsenic*, though it may be thought to contain no phlogiston, really does contain a considerable quantity of it; whereas, if the air be highly dephlogistified, I think it may be considered as the most satisfactory proof we are yet acquainted with, that the substance contains no phlogiston at all.

I shall close this section with an account of the extraction of pure air from other substances

besides *mercurius calcinatus*, and *red lead*, without the addition of spirit of nitre. Of this kind I have only found two substances, viz. *sedative salt*, and *Roman vitriol slightly calcined*, which I had from Dr. Higgins; besides common *salt-petre*, which is known to contain the acid of nitre in itself. From the two former I extracted air by means of a burning lens in quicksilver.

The *sedative salt* is not very manageable in this process; but, with some difficulty, I did extract from it a small quantity of air, in which a candle burned as in common air, and which was diminished as much as common air by nitrous air. At another time, the air which I extracted from this substance was not diminished by nitrous air quite so much as common air is.

N. B. The quantity of air was always very small, not more than the bulk of the materials.

From the *Roman vitriol* I also got but a small quantity of air. The first that I got was diminished by nitrous air, exactly as much as common air. I repeated the experiment, and the air which I then got was diminished by nitrous air considerably more than common air.

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The result of these experiments rather surprizes me, as, after many trials made with a view to it, I could get no such air from any species of *factitious vitriol*, calcined or uncalcined. There must certainly have been some nitrous acid in that Roman vitriol.

The readers of my former publications on this subject will remember, that I was exceedingly puzzled with the experiments which I made to extract air from *salt-petre* in a gun-barrel; the results appearing to me very extraordinary, and well worth attending to, as they might lead to considerable discoveries. See Vol. I. p. 155. In fact, there was sufficient reason for the conjecture; but the method which I then took to extract air from this substance was ill adapted to make it yield its genuine produce. I had not, however, at that time, thought of any other.

The air I first got admitted a candle to burn in it with a very strong flame, and with a crackling noise. Also, though, after having stood a whole year in water, it became quite noxious, yet by agitation in fresh water, it was perfectly restored; so that a candle would burn in it again. At the time of my last publication, I conjectured that this air was *phlogisticated nitrous air*; but now I think it must have been

dephlogisticated air, though produced in a gun-barrel, in which the spirit of nitre, by dissolving the iron, would be very apt to deprave the air; and accordingly, in repeating this experiment some time afterwards, I got air that extinguished a candle.

I was much puzzled, at that time, to account for the very different results of what was, to appearance, the same experiment; but I do not wonder at it now. I imagine that, in the former case, the air was produced very rapidly, and therefore that there was not time for the spirit of nitre to act upon the iron; and consequently the salt-petre gave its natural produce: whereas, in the latter case, a mixture of nitrous air (produced by the solution of iron in the nitrous acid, disengaged from the salt-petre) had thoroughly depraved the air. I advance this with the more certainty, as I have found that salt-petre, heated in a glass-vessel, yields very pure dephlogisticated air, its own earth, and the spirit of nitre which it contains, being capable, by heat, of forming that kind of union of those two principles which the constitution of that air requires; and this, I think, is a pretty remarkable circumstance.

It may be worth while to observe, that I began my experiments upon nitre in quick-silver;

silver; but that the air produced in this manner was nitrous, occasioned by the solution of the quicksilver, as in the former case, by the solution of the iron in the spirit of nitre disengaged in the operation. A copious white fume issued from the nitre in the course of this experiment, like that which attends the rapid production of nitrous air from metals.

When I had recourse to my tall glass-vessels (fig. *d.*) I used an ounce of salt-petre pounded; and filling the vessel up to the mouth with pounded flint, I took the produce of air at nine times, each about three quarters of an ounce-measure. The first produce was not quite so good as common air, the second was of the same degree of purity with common air, the third rather worse; but the fourth was so far dephlogisticated, that one measure of it, and two of nitrous air, occupied the space of one-fifth less than one measure. The fifth produce was still better; for one measure of it, and two of nitrous air, occupied the space of half a measure. The ninth was about the same degree of purity; and the rest, I presume, were not much different.

Being desirous of knowing what kind of air was produced by the explosion of gun-powder, I, for that purpose, mixed equal quantities of
brimstone,

brimstone and salt-petre, both finely pounded, and put them into a tall glass-vessel. The production of air was very rapid and copious, and so highly nitrous, that two measures of common air, and one of this, occupied the space of $2\frac{1}{4}$ measures. Since the produce of air from spirit of nitre and charcoal is the very same with this, viz. nitrous air, it cannot be doubted but that nitrous air is also produced in the explosion of gun-powder, which is composed of those ingredients; the spirit of nitre not being destroyed, or so far decomposed as that its acid nature is lost, but only entering into the composition of this species of air.

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S E C T I O N V.

Miscellaneous Observations on the Properties of
DEPHLOGISTICATED AIR.

I endeavoured, in a variety of ways, to find the specific gravity of dephlogisticated air, by carefully weighing the materials before and after the production; and though this is by no means an exact method of ascertaining this circumstance, and I had recourse to better methods afterwards, the experiments may be worth reciting.

Having put into a gun-barrel two ounces four pennyweights of red lead, I extracted from it twenty ounce-measures of dephlogisticated air, receiving it in water; and the residuum, collected with all the care that I could apply, weighed 1 oz. 16 dwt. 18 gr.; so that twenty ounces of air ought to have weighed 7 dwt. 6 gr. which is beyond all proportion; so that this method must be very uncertain: besides, no allowance was made, nor could well be made, for the fixed air which the red lead yielded, and which is the heaviest species of air that we are yet acquainted with. At
other

other times I have found that red-lead was changed into a real lead, when I was attempting the same thing in this way.

A second attempt came a little nearer the truth. I weighed an ounce of red lead, moistened it with smoking spirit of nitre, and dried it, when it weighed 1 oz. 6 dwt. 12 gr. I then divided the whole quantity into two equal parts, and put one of them into a gun-barrel, in order to collect the air, and the other I put into a crucible, to be exposed to the same degree of heat. The former yielded twenty-two ounce-measures of air, after the fixed air was pretty well washed out of it. It was about five times as good as common air. The latter had lost nineteen grains in weight, being just so much less than half an ounce; so that the twenty-two ounce-measures of air should have weighed nineteen grains, which is certainly a great deal too much: besides, in this experiment, as in the former, no account could be taken of the fixed air.

Finding these methods to fail, I had recourse to that which was used by Mr. Cavendish in weighing fixed and inflammable air, and which is more accurate than the method which I had used before (viz. filling a Florence-flask with the different kinds of air, and weighing them
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in it) because, as the flask must be first filled with water, one cannot be sure, though every possible precaution be taken, that the water has been equally drained from it after each experiment: otherwise there would be a considerable advantage in this method; because the quantity of air may be accurately known. But though this cannot be done with precision in a bladder, as used by Mr. Cavendish, because the degree of distension cannot be measured with much accuracy, yet this circumstance is more than counterbalanced by being able to change the air with compressing the bladder, without wetting it.

I therefore took a glass-tube about nine inches long, and fastening it to the neck of a bladder, which, with such a degree of distension as I could give it, in the manner in which the experiment was made, contained fifty-five ounce-measures, or one pennyweight nine grains of common air. The tube was so fastened, that I could take it out at pleasure; and having the bladder thus prepared, I carefully compressed it, then filling it in part with that kind of air which I was about to weigh, I compressed it again, and then filled it intirely; so that I was pretty confident that the air within the bladder contained very little common, or any other kind of air. In this manner

ner I proceeded to weigh *dephlogisticated air*, and at the same time *nitrous air*, and *air phlogisticated with iron filings and brimstone*, which I take for granted is the same thing with air phlogisticated by any other process.

The following short table exhibits the result of all these experiments at one view.

The bladder, filled with	dwts. gr.		
phlogisticated air, weighed	-	-	7 15
———— nitrous air	-	-	7 16
———— common air	-	-	7 17
———— dephlogisticated air	-	-	7 19

This result agrees sufficiently well with my former observations, though they were not made with so much accuracy, viz. that both nitrous air, and air diminished by phlogistic processes, are rather lighter than common air; and it is consonant to this, that, in the present experiment, dephlogisticated air appears to be a little heavier than common air.

Comparing these observations with that of the extreme lightness of inflammable air; ascertained by Mr. Cavendish, it should seem that the less phlogiston any kind of air contains the heavier it is, and the more phlogiston it contains

contains the lighter it is; though this is by no means the case with solid substances, and indeed it is rather unfavourable to this hypothesis, that nitrous air should not be lighter than dephlogisticated air; for it should seem, by its property of phlogisticating common air, that it should itself contain a greater proportion of phlogiston. Also, in the above mentioned processes for making air, the more phlogiston there is in the substances moistened with spirit of nitre, the more certain it is that the produce will be nitrous air; as the less phlogiston they contain, the more certain it is that the produce will be pure air. But I suspect that there is a farther difference in the *mode* in which phlogiston is combined with spirit of nitre, in the constitution of nitrous air.

In this experiment, the dephlogisticated air was so pure, that one measure of it, and two of nitrous air, occupied the space of $\frac{4}{3}$ of a measure. Had the air been more pure, it would, no doubt, have been specifically heavier still.

It should be observed, that sufficient time ought to be allowed to get dephlogisticated air intirely free from fixed air before it is weighed; and

and as this requires time, and perhaps may never be done completely, it may be suspected that the additional weight of this kind of air is owing to a mixture of fixed air. But common air also contains a great proportion of fixed air, and the dephlogisticated air, on which I made this experiment, had been produced, at least the far greatest part of it, and had been exposed to water, some weeks. It is, however, sufficiently evident, that dephlogisticated air doth become better by standing in water; owing, probably, to its depositing more fixed air in those circumstances.

Having at one time made a large reservoir of dephlogisticated air, for the purpose of experiments, I found that, in about ten days, from being $4\frac{1}{2}$, it had become $5\frac{1}{2}$ better than common air. Standing in pure water must be a surer method of getting the purest dephlogisticated air, than *agitation* in water; for, though the latter method will enable the water to absorb the fixed air faster, and therefore a little agitation at the first will be very useful, in order to expedite the purification of it; yet, as I have found (vol. I. p. 158) that agitation in the purest water will, in time, injure common air; the same operation may be supposed to injure dephlogisticated air also; and indeed I have already observed, that having
agitated

agitated in water a quantity of dephlogisticated air, a candle burned in it only as in common air, and not with that vivid flame with which it burns in this air when it is purer.

I have not made many experiments on the mixture of dephlogisticated air with the other kinds of air, because the analogy which it bears to common air is so great, that I think any person may know before-hand, what the result of such experiments would be. It is pleasing, however, to observe how readily and perfectly dephlogisticated air mixes with phlogisticated air, or air injured by respiration, putrefaction, &c. each tempering the other; so that the purity of the mixture may be accurately known from the quantity and quality of the two kinds of air before mixture. Thus, if one measure of perfectly noxious air be put to one measure of air that is exactly twice as good as common air, the mixture will be precisely of the standard of common air.

I observed also, in making this experiment, that after mixing one measure of each of these kinds of air, they made exactly two measures; so that there was neither any increase nor diminution of quantity in consequence of the mixture, as is the effect of mixing nitrous air with either common or dephlogisticated air.

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It may hence be inferred, that a quantity of very pure air would agreeably qualify the noxious air of a room in which much company should be confined, and which should be so situated, that it could not be conveniently ventilated; so that from being offensive and unwholesome, it would almost instantly become sweet and wholesome. This air might be brought into the room in casks; or a laboratory might be constructed for generating the air, and throwing it into the room as fast as it should be produced. This pure air would be sufficiently cheap for the purpose of many assemblies, and a very little ingenuity would be sufficient to reduce the scheme into practice.

I easily conjectured, that inflammable air would explode with more violence, and a louder report, by the help of dephlogisticated than of common air; but the effect far exceeded my expectations, and it has never failed to surprize every person before whom I have made the experiment.

Inflammable air requires about two-thirds of common air to make it explode to the greatest advantage; and if a phial, containing about an ounce-measure and half, be used for the experiment, the explosion with common air will be so small, as not to be heard farther than, perhaps,

perhaps, fifty or sixty yards; but with little more than one-third of highly dephlogisticated air, and the rest inflammable air, in the same phial, the report will be almost as loud as that of a small pistol; being, to judge by the ear, not less than forty or fifty times as loud as with common air.

The orifice of the phial in which this experiment is made, should not much exceed a quarter of an inch, and the phial should be a very strong one; otherwise it will certainly burst with the explosion. The repercussion is very considerable; and the heat produced by the explosion very sensible to the hand that holds it. I have sometimes amused myself with carrying in my pocket, phials thus charged with a mixture of dephlogisticated and inflammable air, confined either with common corks or ground-stopples, and I have perceived no difference in the explosion, after keeping them a long time, and carrying them to any distance.

The dipping of a lighted candle into a jar filled with dephlogisticated air is alone a very beautiful experiment. The strength and vivacity of the flame is striking, and the heat produced by the flame, in these circumstances is also remarkably great. But this experiment is more pleasing, when the air is only little more

than twice as good as common air; for when it is highly dephlogisticated, the candle burns with a crackling noise, as if it was full of some combustible matter.

It may be inferred, from the very great explosions made in dephlogisticated air, that, were it possible to fire gunpowder in it, less than a tenth part of the charge, in all cases, would suffice; the force of an explosion in this kind of air, far exceeding what might have been expected from the purity of it, as shewn in other kinds of trial. But I do not see how it is possible to make this application of it. I should not, however, think it difficult to confine gunpowder in bladders, with the interstices of the grains filled with this, instead of common air; and such bladders of gunpowder might, perhaps, be used in mines, or for blowing up rocks, in digging for metals, &c.

Nothing, however, would be easier than to augment the force of fire to a prodigious degree; by blowing it with dephlogisticated air instead of common air. This I have tried, in the presence of my friend Mr. Magellan, by filling a bladder with it, and puffing it, through a small glass-tube, upon a piece of lighted wood: but it would be very easy to supply

supply a pair of bellows with it from a large reservoir.

Possibly much greater things might be effected by chymists, in a variety of respects, with the prodigious heat which this air may be the means of affording them. I had no sooner mentioned the discovery of this kind of air to my friend Mr. Michell, than this use of it occurred to him. He observed that possibly *platina* might be melted by means of it.

From the greater strength and vivacity of the flame of a candle, in this pure air, it may be conjectured, that it might be peculiarly salutary to the lungs in certain morbid cases, when the common air would not be sufficient to carry off the phlogistic putrid effluvium fast enough. But, perhaps, we may also infer from these experiments, that though pure dephlogisticated air might be very useful as a *medicine*, it might not be so proper for us in the usual healthy state of the body: for, as a candle burns out much faster in dephlogisticated than in common air, so we might, as may be said, *live out too fast*, and the animal powers be too soon exhausted in this pure kind of air. A moralist, at least, may say, that the air which nature has provided for us is as good as we deserve.

My reader will not wonder, that, after having ascertained the superior goodness of dephlogisticated air by mice living in it, and the other tests above mentioned, I should have the curiosity to taste it myself. I have gratified that curiosity, by breathing it, drawing it through a glass-syphon, and, by this means, I reduced a large jar full of it to the standard of common air. The feeling of it to my lungs was not sensibly different from that of common air; but I fancied that my breast felt peculiarly light and easy for some time afterwards. Who can tell but that, in time, this pure air may become a fashionable article in luxury. Hitherto only two mice and myself have had the privilege of breathing it.

Whether the air of the atmosphere was, in remote times, or will be in future time, better or worse than it is at present, is a curious speculation; but I have no theory to enable me to throw any light upon it. Philosophers, in future time, may easily determine, by comparing their observations with mine, whether the air in general preserves the very same degree of purity, or whether it becomes more or less fit for respiration in a course of time; and also, whether the changes to which it may be subject are *equable*, or otherwise; and by this means may acquire *data*, by which to judge both of
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the past and future state of the atmosphere. But no observations of this kind having been made, in former times, all that any person could now advance on this subject would be little more than random conjecture. If we might be allowed to form any judgment from the length of human life in different ages, which seems to be the only *datum* that is left us for this purpose, we may conclude that, in general, the air of the atmosphere has, for many ages, preserved the same degree of purity. This *datum*, however, is by no means sufficient for an accurate solution of the problem.

SECTION V.

*Of Air procured from various Substances by Means
of Heat only.*

I have observed already, that, in my former experiments, I had not the use of a *burning lens* of any considerable force; and, for want of it, was obliged to leave many of the experiments extremely incomplete, and many things not even attempted. But having, soon after my late publication, provided myself with a lens of sufficient force for the purpose, the first thing I did, when I began to resume my experiments, was to make use of it, in order to satisfy myself what kind of air certain substances would yield by means of heat only, either in *vacuo*, or when confined by quicksilver; and it has been seen in the preceding sections, that by pursuing this method, I was led to the discovery of many new and curious facts, of sufficient importance to be considered separately, and at large.

In this section, I propose to comprize the rest of the observations that occurred to me in
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that course of experiments, intermixed with those which I made in expelling air from the same substances, in a gun-barrel; having sometimes made use of one of the methods, and sometimes of the other, according to different circumstances and views.

These experiments were begun in June, 1774; and one of the first observations that I made was, that inflammable air may be procured from several metals by heat only, without any acid, which was not my opinion at the time of my former publications. I had rather thought that, because, when the marine acid air had decomposed substances containing phlogiston (as brimstone, charcoal, &c.) a quantity of inflammable air was produced, the acid air had contributed to its formation, and had entered into its constitution; and I had therefore inferred universally, that inflammable air consists of acid air and phlogiston. And because inflammable air may be deprived of its inflammability, and from being highly noxious, become respirable by agitation in water, I had farther conjectured, that the air of the atmosphere might consist of the union of acid air and phlogiston; and I do not see how any person could have avoided forming such a conjecture from such premises.

Nor

Nor indeed am I now absolutely certain, that the conclusion was wrong; for the chymical principles are so altered, by combination, that many of them are known to exist where their presence is least of all suspected: unless, however, there be an acid in metals, I do not see how my former opinion can be maintained, in consistency with the facts that will appear in this section, viz. that inflammable air may be procured from several metals confined by quicksilver, with heat from a burning lens, without any solution of the metal in an acid.

It is evident, however, on the other hand, that inflammable air doth not consist of pure phlogiston; because, as I have shewn, it may be intirely deprived of its inflammability; and though it be afterwards diminished in bulk, yet a very great proportion of it remains; being then in the same state with the air in which a candle has burned out, but sufficiently pure for respiration. The question is, *what is the basis of inflammable air*, or what is the chymical principle to which the phlogiston is united in its constitution. In this case it should seem to be some new mode of combination with the earth of the metal. The facts, however, were as follows.

Having

Having put a quantity of *iron-filings*, carefully sorted with a magnet, into one of the glass-vessels, fig. *a*, I filled the rest of the vessel with quicksilver; and placing it inverted in a basin of quicksilver, I threw the focus of the lens upon the iron-filings, and presently air was produced; which, being examined, appeared to be inflammable, though not very strongly so. It resembled inflammable air that had been washed in water till its inflammability was nearly gone. I also could not distinguish the colour of the flame, when I made the explosion in the usual manner, by the approach of a candle. After the operation, the iron from which the air had been extracted, had an exceedingly strong smell, exactly like that of very strong inflammable air procured from metals by acids.

In the same manner I got air from the *filings of watch-springs*, which are made of the best steel; and it was not to be distinguished from the inflammable air of the last experiment. These filings, as well as those of iron, I had carefully sorted with a magnet; so that I believe there was no foreign matter mixed with them. The greatest care, however, is requisite for this purpose, since, the least bit of wood; or any vegetable or animal matter, hardly discoverable by the eye, will yield more inflammable

mable air, than a considerable quantity of iron-filings.

N. B. The spot on which the focus of the lens was thrown, was much blacker than any other part of the filings; and during the application of the heat, a quantity of the filings would sometimes be dispersed, as by an explosion below the surface of them; owing, I suppose, to the sudden generation of air from some of the filings that lay under the rest, but where the heat could reach them.

Having thus got air from *iron*, I proceeded to make similar experiments on other metals. But as all the other metals have more or less affinity with quicksilver, I was obliged to have recourse to a *vacuum*; but being possessed of Mr. Smeaton's air-pump, I could depend upon the vacuum being very exact; so that very little common air could be mixed with the air produced. That the filings of the different metals might be perfectly unmixed, I procured new files, quite clean, and used one side of each for each of the metals.

With this apparatus, I threw the focus of my lens upon filings of *zinc*, and presently got from them air which was very strongly inflammable. Zinc is said to contain more phlogiston
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than the other metals, and the difference between the inflammable air from zinc, and that which I got from iron, was very striking.

From *brass-dust* I got inflammable air in considerable plenty, and also from tin; but this last was very slightly inflammable. I could not have perceived it to be so at all but by dipping a lighted candle into a vessel full of it: whereas, in other cases, I made the trials by presenting the flame of a candle to the narrow mouth of a phial filled with the air. That brass should yield inflammable air, I attribute to the zinc, by the addition of which, copper is converted into brass.

Thus all the metals that yield inflammable air, when dissolved in acids, give inflammable air also by heat only. With other metals I had no success.

Regulus of *Antimony*, heated *in vacuo*, smoked very much, and blackened all the inside of my receiver; but the air that I got from it was very little indeed, and extinguished a candle.

From *bismuth*, and *nickel*, I got hardly any air at all; but in these experiments the heat was not advantageously applied, and the bismuth

muth soon melted into large lumps, on which my lens had no power.

I got no air from *lead* or *copper*. By throwing the focus of the lens upon the former, the receiver was filled with fumes; but the heat was by no means sufficient for the experiment with copper.

In the account of my former experiments, I mentioned one that puzzled me and my friends exceedingly. It was that the air which I got from *chalk*, in a gun-barrel, was inflammable, and burned with a blue flame. I then conjectured, that this property came from the iron; and the experiments that I made with the burning lens have confirmed that conjecture. But why this inflammable air should burn with a *blue flame*, I was long at a loss to account for; since inflammable air from iron only, does not burn in that manner. At length it occurred to me, to try what would be the effect of burning inflammable air mixed with fixed air, procured from calcareous substances by acids; when I found it always burned with a blue flame. This fact I must have seen, perhaps, a hundred times, in a long course of experiments, on the mixture of fixed and inflammable air, made when I was very young in these inquiries, thinking that, together, they

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might

might be common air: but for want of attending to the *colour of the flame*, as not being the object I had then in view, I have been so much puzzled since. I am still at a loss to explain the reason of this effect of the mixture of these two kinds of air.

Fixed air is readily discharged from chalk by any acid; but it is a very small quantity only, that mere *heat* will expel from it. However, from iron-filings and chalk, which I mixed together, in order to resemble the circumstances of the experiment with the chalk in the gun-barrel, I got air in great plenty; and this was exactly of the same kind with that which I had got from chalk in the gun-barrel. Very much of it was inflammable, and it burned with a blue flame. In a second experiment of this kind, I had the same result.

No *calx* of any metal on which I made the experiment yielded inflammable air, but all of them fixed air, and generally in great plenty. *Rust of iron* gave a great deal of air, two-thirds of which was fixed air, and the rest was not affected by nitrous air, and extinguished a candle; so that the whole produce seemed to be fixed air, only with a larger residuum of that part which is not miscible with water than usual. At another time, however, I got from
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the rust of iron fixed air that was very pure, there being little of it that was not miscible with water. It is possible, though I cannot pretend to recollect the circumstances of the experiment, that I might use less heat in the latter case than in the former.

N. B. That part of the rust on which the focus of the lens fell, turned very black.

I observed in a former section, that both the *grey calx of lead*, and *litharge*, yielded fixed air, and that a great quantity of fixed air is contained in *red lead*, and in other preparations of that metal.

I got a little air by means of the burning lens in quicksilver, from *cinnabar prepared with antimony*; but not enough to form a judgment of the quality of it. From common *vermillion* I got more air, viz. about forty times its own bulk, and it was all fixed air, being readily absorbed by water. This substance, like the rust of iron, turned black in the focus of the lens.

The *metallic salts*, if they gave any air at all, gave fixed air, which I find to be contained in most saline substances. I shall recite a few experiments

White lead yielded air in great plenty, by the heat of the burning lens, and it was all pure fixed air.

All the kinds of *copperas* gave fixed air. I first tried common *green copperas* in quicksilver. It dissolved into a great quantity of water, but the air produced from it was not $\frac{1}{20}$ of its bulk. Half of this air was readily absorbed by water, and the remainder was too small to be examined. I repeated the experiment on calcined copperas, both in a gun-barrel, and likewise in a tall glass-vessel filled with sand; but the produce, in all the cases, was fixed air. Half an ounce of calcined copperas yielded near a pint of air.

When I had extracted air from the calx of green copperas in a glass-vessel, I put the same materials into a gun-barrel; but still-I extrac-

ted nothing from them besides fixed air, mixed with acid air, as appeared by the extremely small bubbles to which the large ones were presently reduced in passing through water.

When I made the experiment on *blue vitriol*, which consists of oil of vitriol and copper, in quicksilver, the result was the same as with the green copperas, except that much less water was produced.

White vitriol, which consists of oil of vitriol and zinc, gave ten times as much air as the other kinds. Half of it was absorbed by water, and a candle burned in the remainder. When I extracted air from calcined white copperas, in a glass-vessel, besides fixed air, I got some that diminished common air a little; but I conjecture that this nitrous property must have come from some other substance that was accidentally mixed with the vitriol.

Mercurial nitre gave a great quantity of air in quicksilver, and this was pure nitrous air; but possibly the nitrous acid being let loose from this substance, had produced the nitrous air by dissolving the quicksilver.

From *Roman vitriol revived by flour*, which I had of Dr. Higgins, I got air, half of which
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was fixed, and the remainder was not diminished by nitrous air.

All the air that I was ever able to get from *saline substances* was fixed air. I began with *alum*, and the first experiment that I made upon this substance was with the sun-beams, in quicksilver; when I got from it a little air, which appeared to be fixed air, by extinguishing a candle, and by being readily absorbed by water. I repeated the experiment with the same result. The quantity of air extracted from a piece of alum, was about one-third of its bulk; but I imagined that a little, though not much, more might have been extracted, by a longer continuance of the operation.

I observed, upon this occasion, that I could calcine only a given quantity of alum in a given quantity of air; and that when this was saturated, I could only keep the alum in a fluid state by heat. But it was easily calcined in vacuo; and as the receivers in which the calcination was made became very moist, it is pretty evident that this operation is performed by the mere expulsion of the water which enters into the composition of this salt; so that when the surrounding air can take no more water, that calcination can proceed no farther. I also observed, upon this occasion, that when

I had calcined a quantity of alum in a given quantity of common air, the air was not diminished, or in the smallest degree injured by the operation.

After this, I endeavoured to get air from *calcined alum*, with a burning lens; and I did get a little: but I made no other observation upon it, than that it was not diminished by nitrous air. But when I put a quantity of calcined alum into a gun-barrel, I got from it a considerable quantity of air, part of which was fixed air, precipitating lime in lime-water, and the remainder did not differ from the residuum of fixed air, extinguishing a candle, and neither affecting common air, nor being affected by nitrous air.

From half an ounce of *vitriolated tartar*, in a gun-barrel, I got about $1 \frac{1}{2}$ ounce-measure of air, which was chiefly fixed air. The last produce diminished common air a little; but this I attribute to the gun-barrel, not having been perfectly cleaned from the materials used in a former experiment.

Borax was only melted by the burning lens; but *calcined borax* gave a little air, about its own bulk; and this air extinguished a candle, and was not diminished by nitrous air; so that

it seems to be the same thing with the residuum of fixed air: and this is, in fact, much the same thing, if not quite the same thing, with common air phlogisticated. I was induced to make this experiment, in consequence of that which I had made on sedative salt, which is made from borax, and from which, as I have observed, I had extracted air, about as good as common air; being in hopes that this experiment would throw some light upon the other; but I was disappointed in that expectation.

Having thrown the focus of the burning lens upon a piece of *volatile sal ammoniac*, in quicksilver, a great quantity of air was presently expelled from it; but upon withdrawing the heat, a great part of it soon disappeared, leaving the sides of the vessel covered with slender crystals, exactly like those which are produced by a mixture of fixed air, and alkaline air. The remainder was imbibed by water, being, no doubt, fixed air.

Among other things, I threw the focus of the lens upon a piece of fine *white sugar*, in quicksilver. It was readily melted, and converted into a brown substance, yielding about two-thirds of its bulk of air, one-third of which was readily absorbed by water, and the re-

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mainder extinguished a candle. I repeated the experiment with a brownish powdered sugar, with the same result, excepting that more air was generated from this than from the white sugar, in proportion to their bulks.

From *common salt*, confined by quicksilver, I got no air at all.

There has been a good deal of difference of opinion among philosophers, about the quality of the air that is really contained in chalk. Dr. Black's opinion is, that it is properly fixed air; whereas others have thought that the acid by which the air is dislodged from the chalk, really enters into the air that is produced in the process, and accordingly, that the fixed air produced by different acids, has different properties. An Italian philosopher, who did me the honour to write to me upon the subject, informs me, that he has discovered that air produced from chalk by heat, is of a different nature from that which is got from it by acids, and particularly that the former will not make water acidulous. For my own part, I must acknowledge that I have not examined this subject thoroughly, and have been sometimes inclining to one opinion, and sometimes to another. Sometimes I have thought fixed air to be an *original acid*, and therefore one uniform invariable

riable thing, from whatever substance, and in whatever manner procured. At other times I have been inclined to think, that its acidity is derived from some other acid, especially the nitrous, for reasons that may appear in a subsequent section.

At present, I cannot say that I am quite decided about this question; but that I am much inclined to Dr. Black's opinion, and that all my experiments on chalk are in favour of it. For though I could get but very little air from pure chalk, either in quicksilver, or in vacuo, it was always fixed air, though the residuum was sometimes more considerable than I have found it to be when the air was produced by the solution of chalk in an acid. Once, however, I got a small quantity of very pure fixed air from chalk, by heat, in quicksilver, almost as much of it being absorbed by water, as when chalk is made to give air by means of an acid.

It is remarkable, however, that heat is able to expel but very little air from chalk. I kept a very small quantity of chalk in the focus of my burning lens, which I have observed to be twelve inches in diameter, and twenty inches focal distance, more than half an hour, when the sun was near its greatest altitude, on the

23d of July; but notwithstanding this long exposure to so intense a degree of heat, it seemed to give as much fixed air when thrown into a vessel of water, acidulated with oil of vitriol, as an equal quantity of chalk which had not been exposed to any heat at all. Of this, however, I only judged by the visible effervescence, and did not make any attempt to measure the produce of air, in order to ascertain the effect of these different circumstances with accuracy. I have also kept chalk more than a quarter of an hour in the strongest heat of a smith's forge, in a crucible, without making any sensible alteration in it. But I believe there may be great differences in the constitution of different specimens of chalk in this respect.

When I put a quantity of chalk into a tall glass-vessel, fig. *d*, and kept it in as strong a sand-heat as it would bear, without melting, I extracted from it about its own bulk of air; and examining the state of it at small intervals, I always found that it precipitated lime in lime-water, and that the residuum, not absorbed by water, extinguished a candle: and these seem to be the surest tests of genuine fixed air.

SECTION VII.

Of Air produced by the Solution of Vegetable Substances in Spirit of Nitre.

The experiments, of which an account will be given in this section, were occasioned, in part, by a hint thrown out by Mr. Bewley, in his letter to me, printed in the *Appendix* of my former volume; but more immediately by an experiment which I had the pleasure to see at Paris, in the laboratory of Mr. Lavoisier, my excellent fellow-labourer in these inquiries, and to whom, in a variety of respects, the philosophical part of the world has very great obligations.

Mr. Bewley says, that he had always taken it for granted, that the elastic fluid, generated in the preparation of nitrous ether, without distillation, was fixed air; but that, after seeing the first publication of my papers relating to air, he found, on examination, that it had the general properties of nitrous air.

At Mr. Lavoisier's I saw, with great astonishment, the rapid production of, I believe, near
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two gallons of air, from a mixture of spirit of nitre and spirit of wine, heated with a pan of charcoal; and when that ingenious philosopher drew this air out of the receiver with a pump, and applied the flame of a candle to the orifice of the tube through which it was conveyed into the open air, it burned with a blue flame; and working the pump pretty vigorously, he made the streams of blue flame extend to a considerable distance. Being very much struck with this experiment, I determined with myself to give particular attention to it, and pursue it after my return to England.

My first idea was, that this air was the same thing with the phlogisticated nitrous air which I had procured by exposing pieces of iron or liver of sulphur to nitrous air, the phlogiston of the spirit of wine being, as I supposed, disengaged in this process, and becoming incorporated with the nitrous acid, in the same manner as the phlogiston that is disengaged from the two other substances. These kinds of air differed, however, in one respect, viz. that in Mr. Lavoisier's experiment the flame was blue, whereas it had not been so in mine. But this seemed to be a circumstance of no great importance. Indeed I cannot say, that, at present, my idea of the thing is materially different from what it was then; but I have since
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had an opportunity, by pursuing this experiment, of observing a much greater variety in the production of air by means of spirit of nitre, than I had any expectation of before.

In reality, the nitrous acid is of a most wonderful nature; the more I consider it, the more it excites my admiration, and the more unfathomable the subject appears. I flatter myself that I have made considerable advances in the investigation of it myself, and I still propose to keep it in view; but I own I have very little expectation of seeing it thoroughly explained.

In general, it will be seen, in the course of these experiments, that if the substance with which the spirit of nitre is heated, whether it be fluid or solid, contain much phlogiston, the air produced from it will be nitrous air, or possess the property of diminishing common air to a considerable degree; and, in almost all cases, with a mixture of fixed air. If the substance be inflammable, the air will generally be such as I saw at Mr. Lavoisier's, burning with a blue flame. But this inflammability is of a very delicate kind, resembling that of phlogisticated nitrous air: for the air is easily deprived of it by washing in water.

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A particular account of these experiments, though very remarkable in their nature, will, I foresee, be thought tedious by some persons; but the detail will be very useful to such as shall chuse to prosecute them; especially on account of the precautions that I shall occasionally give to prevent disagreeable accidents from them. Every chymist knows how hazardous it is to mix spirit of nitre with inflammable matters; and I was not unapprized of it, having seen the effect in a course of chymical lectures many years ago. But, being obliged to make these mixtures in a very different manner, the effect could not be obviated without a variety of precautions, which experience only taught me.

Beginning with *spirit of wine*, in imitation of the experiment which I had seen at Mr. Lavoisier's, I made the mixture with the spirit of nitre, in the manner directed in the process for making nitrous ether; putting about one-third of spirit of nitre, to two-thirds of spirit of wine, in such phial as *e*, vol. I. plate I.; mixing them very gradually. Heating this mixture with the flame of a candle, I received the air in water; and when I had procured a considerable quantity of it, I examined it, and found it to burn with a gentle blue, or greenish flame, nearly the same, as well as I could recollect,

recollect, with that which I had seen at Mr. Lavoisier's; so that I had no doubt but that my process, though somewhat different from his, had answered perfectly well.

Considering this flame with attention, I thought it very much resembled that which is produced by a mixture of about one-third inflammable air, and two-thirds nitrous air; and concluded, that it was probably composed of them both; the nitrous acid forming nitrous air, by seizing upon the phlogiston of the spirit of wine; and there being a redundancy of inflammable matter, sufficient to render the air partially inflammable.

In the directions to make nitrous ether, I was cautioned to pour the spirit of nitre upon the spirit of wine, and by no means to pour the spirit of wine upon the spirit of nitre. But though this method of mixing these liquids may not answer the purpose of making nitrous ether, it answered very well for the production of air, and was a very useful variety in the process. It is necessary, however, that the unexperienced operator should be upon his guard in these experiments.

The spirit of nitre should be much diluted, and the quantity of any liquid inflammable
matter

matter should be very small, just sufficient to cover the surface of it: otherwise, though the mixture may exhibit no alarming appearance at first, it will, in a little time, become very black, beginning at the surface; the phial will then be filled with red fumes, the air will be generated in a prodigious torrent, and, unless the tube through which it is transmitted be sufficiently wide, and the vessel in which the mixture is made be very strong, the whole will be exploded with great violence. Of this I have seen but too many instances; and sometimes when I had thought that my experience had taught me sufficient precaution. Besides, all oily matters become extremely viscid, by mixing with spirit of nitre; and this viscid matter getting into the tube, stops it up, and much increases the hazard of an explosion. But to recur to the experiments.

Having poured a very little *spirit of wine* upon a quantity of diluted spirit of nitre in a glass-phial, with a ground stopple and tube, a great quantity of air was presently produced. When a candle was dipped into this air, it was extinguished; but in going out was surrounded with a slight blue or green flame, but hardly more than is perceived in nitrous air. Almost one-half of this produce of air was readily

dily absorbed by water, and precipitated lime in lime-water; and I doubt not but that, in the subsequent experiments, as well as in this, a great proportion of the air produced in this manner was fixed air. The remainder was nitrous, almost as strong as any.

Upon air produced in this manner from *oil of turpentine*, I happened to make a few more experiments, some of which are not a little remarkable. When I used the strongest spirit of nitre in this process, it was very difficult to get much air, on account of the suddenness of the effervescence; but a great quantity of air is easily produced by diluting the smoking spirit of nitre with an equal quantity of water. At one time, however, when I had heated this mixture pretty much, and it had yielded a great deal of air, though I withdrew the candle, the air continued to be produced faster and faster for about a minute. It then came quite in a torrent; all the oil of turpentine was thrown out of the phial, and the spirit of nitre only left in it. This is likewise the case with other similar mixtures; so that when it is necessary to apply heat, it should be done very gradually and cautiously, and the air should never be generated very fast, unless the purpose of the experiment require it, and the operator be upon his guard accordingly.

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When I received this air in water, it extinguished a candle, and did not diminish common air. When received in quicksilver, it still extinguished a candle; but as it went out the third or fourth time, it was surrounded with a bluish flame, as large as that of the candle. And happening, at one time, to apply more heat than I intended when the air was received in water (and in consequence of it, the air was produced very suddenly) I examined it immediately, and a candle burned in it with an enlarged flame, though not remarkably so. It shows, however, that in this process also, as well as in the process for making phlogisticated nitrous air, the property of its admitting a candle to burn with an enlarged flame depends, in a great measure, upon the *time* at which the experiment is tried after the air is produced, and upon other delicate circumstances.

A quantity of this air, received in water, was about half-absorbed in one night. By agitation it appeared to be absorbed not so readily as fixed air, nor with so much difficulty as nitrous air, but in a medium between both. When this air was reduced to about one-eighth of its original bulk, it was diminished by nitrous air. But this is the case with all the kinds of air that will bear the experiment, and
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even with nitrous air itself, as I have observed in my former publication.

At the time that I made the preceding experiments with oil of turpentine, I had no lime-water at hand ; and therefore only judged that part of the produce was fixed air, by the manner in which it was absorbed by water. But, less certain as this test is, a person much used to experiments of this kind, will be able to apply it with sufficient certainty in most cases. However, repeating this experiment, when I had procured the glass-phials with ground-stopples and tubes, I found that the greatest part of this air was unquestionably fixed air, precipitating lime in lime-water, as much as any fixed air whatever, and that the remainder was strongly nitrous. Attempting at this time also, to receive the air in quicksilver, a good deal of the vapour of the spirit of nitre came over ; and, dissolving the quicksilver, made the produce of air almost wholly nitrous.

I observed, at one time, when I had produced this air in a phial with a ground-stopple, that after the first part of the process, in which no heat was applied, the water rushed back into the phial. Upon this I applied the flame of a candle to the diluted mixture, and getting

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a second produce of air, examined them both separately. Both of them contained a great proportion of fixed air, precipitating lime in lime-water very much; and when the fixed air was washed out of them, they both diminished common air, but the latter more than the former. Two measures of common air, and one of this, occupied the space of little more than two measures.

In order to judge how far an *acid* prevailed in this air from spirit of nitre, and oil of turpentine, I put alkaline air to it; when instantly a white cloud was produced, which rose to the top of the vessel; but it was by no means so dense as that which is produced by mixing alkaline air with any of the acid airs; nor did the whole quantity of air disappear, but only half of it. However, all the inside of the tube was covered with a saline substance, which I did not examine, but supposed it to have been the *nitrous ammoniac*. Having the curiosity to dip the flame of a candle, which happened to be at hand, into the air that remained of this mixture, it appeared to be so far inflammable, as even to make a considerable explosion; but not quite so great a one as I have observed to have been made by a quantity of phlogisticated nitrous air, vol. I. p. 217.

Repeating

Repeating this experiment some time afterwards, about one-fourth of the mixture of this air, and alkaline air, disappeared upon their being put together. Half of the remainder was absorbed by water; and in this second remainder, which, by its redness, on being exposed to common air, appeared to be considerably nitrous: a candle burned with a beautifully enlarged flame.

In these cases the alkaline air must have supplied the phlogiston, which the iron and liver of sulphur had before supplied to nitrous air; in consequence of which it admitted a candle to burn in it in the same manner; for neither of the component parts of this air, viz. the fixed or the nitrous, are either separately, or together, inflammable. It is something remarkable, however, that when I mixed equal quantities of nitrous and alkaline air, and examined the mixture immediately, the nitrous air seemed not to have been at all affected by the alkaline air. It was not in the smallest degree inflammable. I had imagined that alkaline air might, in this manner also, have phlogisticated the nitrous air; but it seems that when it is so applied, it has no such effect.

Air produced from all the *essential oils* by spirit of nitre, has, I believe, the same properties as that which is produced from oil of turpentine. I tried another, but I forget which, in a phial with a ground-stopper, and the air produced from it precipitated lime in lime-water, extinguished a candle, and diminished common air a little.

Ether, both vitriolic and nitrous, heated in spirit of nitre, yields the same kind of air as the essential oils, or spirit of wine, viz. partly fixed air, and partly phlogisticated nitrous air. Equal caution is also necessary in conducting this process; for the phenomena attending it are the same that I described in the beginning of this section, and in the highest degree. I would therefore recommend the using of a very small quantity of the ether, and putting it upon the spirit of nitre.

At first, however, in imitation of the process for making nitrous ether, I poured the spirit of nitre upon the ether, as I had done at first also with spirit of wine; and, heating the mixture, received the air, which it yielded in great plenty, in quicksilver. This air made no cloud with the mixture of alkaline air; it burned exactly like the vapour of ether itself; and when part of the mixture had boiled over,

it quickly absorbed the air that had been generated.

Seeing sufficient reason to disapprove of this process, I had recourse to the other, and found that when I used a very diluted spirit of nitre, and but little ether, the experiment was much more manageable, and the air was produced in sufficient plenty. This air was readily absorbed by water; and upon putting alkaline air to it, a very slight cloud rose to the top of the vessel; but there was no sensible diminution of the quantity of air occasioned by it. When a candle was dipped into this air, it was extinguished many times, but always with a beautiful bluish flame, much larger than the natural flame of the candle. Towards the close of the experiment, the air in the inside of the vessel became red; a certain sign of its being considerably nitrous. On repeating this experiment, when I had procured the phials with the ground-stopples and tubes, I had the most satisfactory proof, that part of this produce of air was fixed air, by its precipitating lime in lime-water; and that the remainder was nitrous, almost as strong as any, by its power of diminishing common air.

The result of the experiment with *nitrous ether* was, in all respects, the very same as that

of this with vitriolic ether. I made the experiment, because it might have been expected that there would have been some difference in the result, as the nitrous ether is the produce of spirit of nitre, with which it was now mixed.

Spirit of nitre, heated with *olive-oil*, yields the same kind of air with that which is produced from essential oils, &c. but the process is exceedingly troublesome, owing to the tenacity of the oil; and it is not much more manageable, when but a very little of the oil is put to a large quantity of the diluted spirit of nitre. The air which I got in this manner precipitated lime in lime-water.

With very great difficulty I got, in a phial with a ground-stopper, a very small quantity of air from spirit of nitre and *tallow*, the water rushing into the vessel after every gush of air. It precipitated lime in lime-water.

The result of the experiment with *bees-wax*, was the very same with that with tallow. Putting a small piece of bees-wax upon a quantity of pretty strong spirit of nitre, I got air which made lime-water turbid; but not enough to ascertain its other properties. This process was equally difficult with the preceding, on
account

account of the water rushing into the phial after every gush of air.

I had the curiosity to endeavour to procure air from some of the *gums*, &c. by this process, and found the result to be, in the main, the same with that of the preceding experiments.

Gum-arabic easily dissolves in the nitrous acid; and as it dissolves, a great quantity of air is produced, making a beautiful appearance; but when the acid is nearly saturated, it becomes viscid, and the vessel gets full of froth. Part of this air was fixed, precipitating lime in lime-water, and being readily absorbed by water. The remainder was nitrous, almost as strong as any.

The result was the same with *gum-copal*, excepting that this substance did not sink in the spirit of nitre, as the gum-arabic had done.

Camphor, with diluted spirit of nitre, yielded very strong nitrous air; but required a considerable degree of heat. A good deal of the camphor, which had been fluid, and had swum on the surface of the spirit of nitre, came over, and resumed its natural appearance in water. I did not try whether any part of this produce was fixed air.

I got some air by spirit of nitre from *amber*, which precipitated lime in lime-water; but the quantity was too small to be examined any farther. Afterwards I got a larger quantity from a greater number of small pieces of amber, heated in a weak spirit of nitre, contained in a phial with a ground-stopper. About one-third of this produce was fixed air, precipitating lime in lime-water, and being readily absorbed by water. In the remainder a candle burned with an enlarged greenish flame. It also diminished common air; so that two measures of common air, and one of this, occupied the space of $2\frac{1}{4}$ measures.

N. B. Most of the pieces of amber used in this experiment were turned black quite through; the rest continuing of their natural colour.

It happened, in the course of these experiments, that a bit of *sealing-wax* got into the phial, and I observed air to issue from it very copiously. Upon this, I put a piece of sealing-wax into the phial, with spirit of nitre, and received the air at different times. That which came over first was, in the highest degree, nitrous; but when, with the application of more heat, I caused a copious production of a very turbid kind of air (which however, presently became transparent) it hardly affected common
air

air at all. It was then pretty readily absorbed by water; and though at first it extinguished a candle, yet when it had been washed in water, a candle burned in it with a blue flame. Indeed when the candle was extinguished in it, it went out with that kind of blue flame. The course of this experiment will be found to be analogous to that with other *hard substances* containing phlogiston, which I shall now recite, though many of them were made before this.

Having found that *charcoal* would dissolve in oil of vitriol, and thereby yield a vitriolic acid air, I had the curiosity to try what would be the effect of an attempt to dissolve this substance in spirit of nitre. This was when I had made but little progress in the preceding experiments with oily and gummy substances, and I had no expectation of the result. I began with taking the produce in quicksilver, as I had done with that from the vitriolic acid; but all that came over in this manner, was the nitrous acid vapour, which, seizing upon the quicksilver, produced nitrous air.

After this, I received the produce in water, and found it to be genuine nitrous air, almost as strong as any that is produced from metals. At that time I was much surprized at this result,

fult, having imagined that nitrous air could not be procured but by the solution of metals in spirit of nitre; and I considered this as another property in which metals and charcoal resemble each other; besides those which I had noted before, and an account of which may be seen in a paper formerly printed in the Philosophical Transactions, and which I shall insert in this volume. But presently after this I got nitrous air equally strong from other hard substances, such as *dry wood* of various kinds, &c.; but in these processes, the quality of the air differs exceedingly, according to the *degree of heat* applied, and other circumstances: and I think the subject deserves a farther investigation. To promote this, I shall recite the principal facts of this kind that have occurred to my observation,

Having poured about a quarter of an ounce-measure of smoking spirit of nitre, mixed with an equal quantity of water, upon some *pounded charcoal*, and having applied to it the flame of a candle, I collected a large jar full of air, in all twenty-eight ounce-measures. When about half of this quantity of air was produced, it was impossible to apply any more heat, but the spirit of nitre would come over; which it did, tinged with a deep black. When all the liquor was come over, still one-fourth part
of

of the air was produced with the application of a strong heat. The air of this whole produce, which was not taken at different times, was strongly nitrous. Two measures of common air, and one of this, occupied the space of no more than two measures.

It was my seeing this air produced in different circumstances, viz. before any of the acid came over, and afterwards, that suggested to me the importance of taking the air at different times, according to the change of circumstances in the production of it; a hint which I pursued to very great advantage afterwards, as the reader has already seen, and will see farther; in the course of my experiments.

Repeating the experiment with this view, I examined the first produce of air, which came over, while the heat was very moderate, and found it to be very strong nitrous air, almost as strong as that which is procured from metals. Towards the last I increased the heat, and by that means produced a very turbid air, of which I collected a prodigious quantity. Sometimes, however, the air would be quite transparent, and then turbid again, several times. I endeavoured to take the turbid air and the transparent separately, and I succeeded pretty well; but I found them both
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to be of the same quality, extinguishing a candle, and diminishing common air but very little; two measures of common air; and one of this, occupying the space of little less than three measures.

At this time I made use of the phials represented fig. *a*, with common corks; and observing that the corks were always much corroded in these experiments, I thought it would be proper to ascertain the effect of the spirit of nitre on the *cork*, in order to make proper allowance for this circumstance in future experiments. I therefore poured a quantity of spirit of nitre upon some pieces of cork, and treating it in the manner above mentioned, I found the produce of air to correspond very exactly with that which I had got from the charcoal. With a moderate degree of heat the air was strongly nitrous; and with a great heat the air was turbid, and much less nitrous. I was not a little surprized to find that nitrous air was produced from cork, as it intirely overturned my system of the production of this air, depending upon that property of the charcoal by which it resembles metals. However, I presently found, that genuine nitrous air was produced from a variety of other *hard substances*; for at that time I had not discovered that it was produced from any liquid ones.

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The correspondence of an experiment which I made with old dry oak with that which I made with charcoal is striking enough; and one of them may a little illustrate the other.

I put about half an ounce measure of the raspings of *old dry oak* into one of the phials above mentioned, fig. *a*, and poured upon them as much spirit of nitre, half-diluted with water, as made them thoroughly moist. Air was instantly produced, without the application of any heat. This air I received, together with a little that was produced by holding the flame of a candle, at the distance of about a quarter of an inch, from the side of the phial. I then placed the candle nearer, and received the air at five different times; the last but one being produced when the flame touched the side of the phial, and the last, when it was placed close under it, and after all the moisture seemed to be expelled from the phial. The first produce was of the nature of nitrous air, the two next much more so, almost as strong as any; but the two last were hardly nitrous at all. A candle went out in this air, burning with a bluish flame, as if it had been in part a mixture of inflammable, nitrous, and fixed air. That part of this produce was fixed air, was evident, by its being readily absorbed
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by water; but I did not apply to it the test of lime-water.

Seeing this astonishing difference in the produce of air by spirit of nitre from different substances, and even from the same substance in different circumstances, I thought that it might be possible, by this means, to distinguish those substances that are *nutritious* from those that are not; and, in my imagination, I had thought it possible to ascertain the *quantity of nutriment* that different substances would yield by the quality and quantity of the air produced from them; but the experiments by no means answered such fond expectations. I found, however, what I did not expect, viz. a most remarkable difference between the air produced from *animal substances* of several kinds, and from *vegetables*; for, in general, the former had little of the nitrous property; but the latter, though nutritious, yielded the same kind of air with that which I had got from wood or charcoal. The facts surprized me very much and I can give the reader no clue to lead him through the labyrinth.

The vegetable substances which I tried were *wheat-flour, barley, and malt*, all of which yielded nitrous air in the first part of the produce, and air of the same quality with the last produce

produce from charcoal, if the process was continued a long time, and with a strong heat. I had once suspected that the nitrous quality might have come from the cork with which the phial was closed; but I was satisfied that it came from the substance within the phial, when, instead of a phial closed with a cork as before, I used one of those represented fig. *b*, which I have observed to have been contrived by Mr. Vaughan. Having put the barley and spirit of nitre into this vessel, I heated it in a vessel full of water, placed on the fire, covering the phial with a glass jar filled with water, in order to receive the air. The air procured in this manner was still strongly nitrous, though it could come from nothing but the spirit of nitre and barley.

As I attended to a few collateral circumstances in the experiment with the *malt*, it may be worth while to recite the particulars. Having just covered one pennyweight of malt with diluted spirit of nitre, I made it boil, and procured from it two jars full of air, each containing near thirty ounce-measures, and I might have collected more. That which came first, and which was transparent, diminished common air almost as much as the strongest nitrous air. The air which came last, and which was turbid, hardly diminished common air at all,
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and it was readily absorbed by water. Before it was agitated in water, it extinguished a candle; but afterwards, when it was reduced to about one-fourth of its original quantity, a candle burned in it with a lambent blue flame.

N. B. Towards the close of this process, part of the contents of the phial were reduced to a coal.

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SECTION VIII.

*Of Air procured by the Solution of ANIMAL
SUBSTANCES in Spirit of Nitre.*

I profess not to be able to assign any reason for the difference in the produce of air from *animal* and *vegetable* substances; but the experiments, of which an account will be given in this section, compared with those recited in the last, will prove, that, in general, there is a very considerable one.

It has been seen that vegetable substances, dissolved in spirit of nitre, besides fixed air, yielded nitrous air, and frequently as strong as that which is procured by the solution of metals in the same acid; and this is the case whether the spirit of nitre be much concentrated, or much diluted. On the contrary, animal substances, in general, treated in the same manner, yield about the same proportion of fixed air; but the residuum is either not at all, or in a very slight degree, nitrous (except in some cases where the spirit of nitre is very strong) but is a kind of air which, neither affecting common air, nor being affected by ni-

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trous air, but simply extinguishing a candle, may be termed *phlogisticated air*. Towards the end of a process, indeed, when, by means of a strong heat, the produce of air is very rapid, and the air full of clouds, it is, like air produced from vegetable substances in the same circumstances, slightly inflammable, burning with a lambent, greenish, or bluish flame.

As there is a considerable variety in the result of these processes, arising from several circumstances, the influence of which may not be apprehended, I have been careful to note every thing relating to them, that appeared to me at the time to be of any importance. But, notwithstanding this, it is very possible I may have made omissions, of the effect of which I was not apprized; and therefore those who shall endeavour to repeat the experiments after me may not find precisely the same results that I have reported. This will often be the case in experimental inquiries so new as these; and as no human care has yet been sufficient to prevent this inconvenience, it is the part of human candour to make proper allowance for it.

I cannot help flattering myself, however, that these experiments, properly pursued, may be a means of throwing light upon the two great natural processes of *vegetation* and *animalization*;

lization; as they exhibit a new and striking difference between substances formed by them. On this account I would willingly recommend them to the particular attention of chymists and physicians. The experiments themselves, nearly in the order in which they were made, are as follows.

I put equal quantities of spirit of nitre and water upon some pieces of *beef*, dried till they were perfectly hard, but without being burned, and took the first produce of the air, which was generated without the application of heat, and was very considerable; and afterwards that which came over when the flame of a candle was placed within about a quarter of an inch from the phial; but neither of them sensibly affected common air. They were both pretty readily absorbed by water, and extinguished a candle. I had expected that this air, like that from dry wood, would have been nitrous air.

This experiment being made with the fleshy part of a *muscle*, I next took a *tendon* from a neck of veal, imagining, from its firmer texture, that the air produced from it might approach nearer to that from wood; but the air that came from it neither diminished common air, nor was diminished by nitrous air, nor was

it readily absorbed by water, and a candle went out in it. It seemed, upon the whole, to be much the same thing with phlogisticated common air.

I thought there might be some difference in this respect, between air produced from the *white*, and from the *brown* flesh of animals; but I made the experiment with the breast and the leg of a *turkey*, without finding any. That which was produced from these substances exactly resembled the air that I had got from the tendon of the calf; except that it was more readily imbibed by water. I agitated a quantity of it in water five minutes, when one-fourth of it was absorbed, but the remainder still extinguished a candle, and did not differ from what it was before, except that it was now diminished by nitrous air, like all other kinds of air agitated in water. When all the flesh was dissolved, air was still produced in great plenty, upon the application of the flame of a candle. The air produced in this manner was very turbid at first; but the quality of it was not sensibly different from that which came first, and which was transparent.

I repeated this experiment with the same event, observing that the turbidness of the air depended upon the *degree of heat* with which

it was produced ; for, after producing a large quantity of turbid air, I lessened the heat, and presently the air was transparent as at first, and upon increasing the heat, the air was turbid again.

Having found no air of the nitrous kind from the flesh of an animal of the *quadruped* species, or of a fowl, I was willing to try what would be the produce from the flesh of *fishes*, *insects*, and *exanguious* animals.

From the flesh of *salmon*, made thoroughly dry, and then dissolved in spirit of nitre, I got a great quantity of air, at first without heat, till the whole was nearly dissolved ; when about a quarter of an ounce measure of this solution still yielded more than a quart of air. At the last this liquor, which had been pretty clear, became suddenly opaque ; and in this state it yielded air the most plentifully, and continued to do so till, all the moisture being evaporated, it became a dry coal. While it continued clear, a strong heat, occasioned by applying the flame of a candle close to the phial, would immediately make the air turbid, especially toward the end of the process, just before the liquor became opaque. At this time, however, the air in the inside of the phial had nothing of that appearance, nothing being seen

in it but the red fumes of the spirit of nitre; but when the liquor became opake, it was filled with very dense white fumes.

The air, in all the stages of this experiment, was in part fixed, precipitating lime in lime-water. In the middle of the process the residuum was nitrous; but only in a slight degree. Towards the conclusion it had no sensible effect on common air; and at last it burned with a blue lambent flame, which continued a considerable time after I had withdrawn the candle by which it had been set on fire. In the air that came just before the last, a candle barely went out, surrounded by a slight flame of that colour.

Repeating the experiment, I found nothing nitrous, either in the first produce of air, before the flesh was dissolved, or afterwards; and at this time I was particularly careful not to use any of the flesh that was turned black, or very brown, in drying; having some suspicion that the nitrous property of the air in the preceding experiment came from such parts of the flesh, being then a kind of charcoal.

The flesh of salmon having a peculiar colour and flavour, I thought it would not be amiss to repeat the experiment with some other kind

kind of fish, the flesh of which was white and tasteless. I therefore took the *flesh of perch*, and dissolving it in spirit of nitre, I procured a large quantity of air, no part of which was nitrous; but a considerable part of it was fixed, precipitating lime in lime-water. The greatest part of this air was produced after all the flesh was dissolved; and at the last, when I increased the heat, the air was turbid; but it did not sensibly differ from that which was produced at first, except that a candle went out in it with the flame slightly tinged with green.

A large *worm*, treated in the same manner, yielded air that was in part fixed, making lime-water turbid. The residuum extinguished a candle, and was, in a small degree, nitrous; owing, perhaps, to something on its stomach; for I had only pressed out the contents with my finger.

Air produced from a number of *wasps*, dissolved in spirit of nitre, was partly fixed, and the residuum so far nitrous, that two measures of common air, and one of this, occupied the space of $2\frac{1}{2}$ measures. When the flame of a candle was dipped into it, it burned with a greenish lambent flame.

I had next the curiosity to try what kind of air might be procured from the insensible *excrescences of animal bodies*, as *horn, hair, feathers*, &c. which are protruded from the body, and seem, at first sight, to be in a kind of intermediate state between vegetable and animal substances; but they appeared to be more of an animal than of a vegetable nature, i. e. judging by the air which I had hitherto found those substances to give.

With spirit of nitre and *hair* I got a quantity of air, part of which was fixed, precipitating lime in lime-water, and the remainder, not absorbed by water, which was about two-thirds of the whole, was in a small degree nitrous.

From a *crow-quill* I got air of the same quality with that from the hair in the preceding experiment. This quill was black; and thinking it possible (as the hair I had made use of was also in part black) that the nitrous property of the air might come from the phlogiston which produced that colour, I repeated the experiment with a *white feather*; but the result was the same: or, rather, the air in this case, was more nitrous than in the former. Two measures of common air, and one of this, occupied the space of $2\frac{1}{2}$ measures. Had I
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used a much diluted spirit of nitre, it will appear probable, from the experiments recited at the close of this section, that the produce would have been less nitrous.

Air was easily procured by dissolving *horn* in spirit of nitre. Part of it was fixed air, precipitating lime in lime-water; but a very great proportion of it was not absorbed by water. In this residuum there was nothing sensibly nitrous. That which came first extinguished a candle, without any particular appearance; but in that which came last, it burned with a beautifully blue lambent flame.

I had thought that, possibly, the *inside of an oyster-shell*, or *mother of pearl*, might, together with fixed air, yield a quantity of such phlogisticated air as had been produced in the preceding experiments; but when they were dissolved in spirit of nitre, they each of them gave very pure fixed air, without any greater residuum than is found in the solution of chalk in oil of vitriol.

Pieces of *ivory* dissolved in a very beautiful manner, in hot spirit of nitre, and yielded a great quantity of air, which, in every stage of the process, precipitated lime in lime-water. The residuum was not nitrous, and extinguished

guished a candle, without any particular colour of the flame.

To try the difference between the same substance, in a natural state, and after it was reduced to a coal by fire, I dissolved some *char-coal of ivory* in spirit of nitre, and found that it yielded plenty of air, the greatest part of which was fixed, and the residuum was considerably nitrous. When the air was produced very fast, the inside of the phial was filled with a white fume. This ivory had been kept in a red heat, covered with sand, about an hour.

Eggs do not rank with the substances above mentioned; but being the produce of an animal, and yet no proper part of one, I shall recite the experiments I made upon them in this place. Both the *white* and the *yolk* of an egg, which I tried separately, yielded a considerable quantity of air, when dissolved in spirit of nitre, and the difference between them was not sensible. In both cases part of the air was fixed, precipitating lime in lime-water, and the residuum was so far nitrous, that two measures of common air, and one of this, occupied the space of $2\frac{1}{2}$ measures.

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It occurred to me, that, possibly, other parts of the animal, and the different animal *secretions*, might yield a different kind of air from that which the muscles had yielded; and from the little that I have done in this way, I cannot help thinking, that the experiments deserve to be prosecuted farther.

From the *crassamentum of blood*, with spirit of nitre, I got great plenty of air, part of which was fixed, but no part nitrous. At last the air was turbid; and, as is usual in this case, a greater proportion of it was fixed air. Towards the last also, when the blood was completely dissolved, the air was produced irregularly; for after an interval of about a quarter of a minute, there would be a sudden gush of about a quarter of an ounce-measure of air; but between these intervals the produce was equable.

Spirit of nitre put to the *serum of blood*, immediately turns it into a white coagulum. This yielded less air than most other substances, treated in the same manner. Part of it was fixed air, precipitating lime in lime-water, and the residuum was not nitrous, and extinguished a candle without any particular appearance.

Milk

Milk was also immediately coagulated by a mixture of strong spirit of nitre, and yielded air, one-third of which was fixed, precipitating lime in lime-water; and the remainder was so far nitrous, that two measures of common air, and one of this, occupied the space of $2\frac{1}{4}$ measures.

From *cheese*, which was pretty old, I got air, a great part of which was fixed, and the remainder considerably nitrous,

Mutton-gravy, with strong spirit of nitre, gave but little air, perhaps twenty times as much as its bulk. It was in part fixed, and the residuum not sensibly *nitrous*.

It has been seen, in a preceding section, that all *oily matters*, of a vegetable nature, yield nitrous air in very great plenty, and that the produce is exceedingly rapid; so that many precautions are necessary in conducting the experiments. On this account I began to use the same in my attempts to get air from *Hog's-lard*, but found them to be altogether unnecessary: for this substance is but little affected by very strong and hot spirit of nitre, on the surface of which it continues fluid, and yields but little air, perhaps four times its bulk. Part of this was fixed air, precipitating lime
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in lime-water, and the remainder was so far nitrous, that two measures of common air, and one of this, occupied the space of less than two measures; that is, it was almost as strongly nitrous as that which is produced from metals.

It is something remarkable, that, of all animal substances on which I have made the experiment, that part which seems to be the most remote from a vegetable nature, and is peculiar to animals, should approach the nearest to the nature of a vegetable in the air which it yields when dissolved in spirit of nitre. This is the *medullary substance of the brain*.

From part of the *brain of a sheep*, dissolved in strong spirit of nitre, I got a quantity of air, about half of which was fixed air, precipitating lime in lime-water, and the remainder was so far nitrous, that two measures of common air, and one of this, occupied the space of $2\frac{1}{4}$ measures. When it was completely dissolved, and by a strong heat, the air came over very turbid, and a candle burned in it with a lam-bent greenish flame.

I repeated the experiment with part of the same brain that was *boiled*, and with the same result; except that I did not continue the process so long. The residuum of this air, when the fixed air was washed out of it, was so much
nitrous,

nitrous, that two measures of common air, and one of this, occupied the space of $2\frac{1}{3}$ measures. This I tried with the last of the three portions of air that I took. The first and second were not so highly nitrous; and yet I am confident that all the three portions were wholly the produce of the solution, both the phial and the tube being filled with bubbles, in the form of froth, before I took any air at all.

After I had made these experiments, it occurred to me, that possibly, this difference in the produce of air from vegetable and animal substances might arise from some difference in the spirit of nitre. But though I found that, in consequence of the acid being more concentrated, or more diluted with water, a real difference was occasioned in the air, still very much depended upon the substances themselves, as will appear from the following experiments.

A piece of *boiled mutton*, dissolved in very strong spirit of nitre, yielded air, which was partly fixed, with the residuum so far nitrous, that two measures of common air, and one of this, occupied the space of $2\frac{1}{3}$ measures. Dissolving a quantity of the same mutton, in the same spirit of nitre, diluted with an equal quantity of distilled water, I procured air, which
was

was not half so much nitrous as that in the preceding experiment. With the same result I also made this experiment with the *white of an egg*, which gave air much less nitrous when dissolved in a diluted spirit of nitre, than in the former case.

In order farther to satisfy myself, whether the result would not be the same with *vegetable* substances also, I took some pieces of very *dry old oak*, and dissolved them in exceedingly weak spirit of nitre. I also caused the air, by means of heat, to be produced very rapidly; in which case the air is generally less nitrous, at least toward the close of an experiment, as the reader will have observed: but when the fixed air was washed out of it, the residuum was almost as strongly nitrous as any air that is produced by the solution of metals.

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SECTION IX.

Miscellaneous Experiments relating to NITRE,
the NITROUS ACID and NITROUS AIR.

I have more than once recommended the study of the *nitrous acid*, and of its various combinations, as promising a fund of valuable discoveries, looking far into the constitution of nature; and I flatter myself, that even my own experiments relating to this subject, recited in this volume, will be thought to have sufficiently verified the observation. But I consider this ample field of inquiry as barely opening, and that much more remains to be done; and considering how easily this rich mine has been worked hitherto, I think one may fairly presume that it will still abundantly reward the diligent labourer.

It has been the opinion of several eminent chymists, that there is but one *primitive acid*; that all the different acids with which we are acquainted, are only different modifications or combinations of it; and that the *nitrous acid*, in particular, differs from the rest by a more intimate union of phlogiston with it.

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The celebrated Mr. Stahl observed, that by distilling iron in the marine acid, some nitrous acid was produced. Mr. Woulfe informs me, that he has done the same by a process different from that of Stahl; and also that he has converted the acid of nitre into the marine acid, which I believe was never done before.

This I consider as a capital discovery; and whenever this excellent chymist shall think proper to publish his process, it will, I doubt not, be a great means of advancing the bounds of natural knowledge.

The relation that the nitrous acid bears to phlogiston is to me, I acknowledge, a great mystery. That this acid always contains phlogiston is very evident; and yet, such is its avidity, as we may say, for more, that it takes phlogiston from most other substances. It is, I presume, by means of this property, that many substances into which the nitrous acid enters, can burn without the assistance of common air; and I now suspect, that it is by the same property that common air itself (which I think I have proved to consist of nitrous acid and earth) is capable of sustaining both flame and animal life.

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I have sufficient proof, however, that the nitrous acid, both when combined, as it generally is, with water, and likewise when exhibited in the form of vapour, or air, is so loaded with phlogiston, as to be capable of phlogisticating both the common air, and the nitrous air, which are exposed to it. This I think to be a fact of a pretty extraordinary kind; at least it appeared so much so to me, that I had expected the very contrary effect from the experiments that I made upon it; having imagined that, since the nitrous acid constitutes the purest of all the kinds of air, common air wanted nothing more than a greater proportion of this acid to become dephlogisticated air; and thus I was in hopes of being master of a process, by which I could not only restore vitiated air to its original purity, but even improve the purity of common air itself. And the attempt is of such a nature, that I am far from thinking that any person needs to despair of succeeding in it, though the method which I took to accomplish it did not answer, but had a contrary effect.

Among my random experiments to restore vitiated air, I have mentioned my having exposed it to the fumes of smoking spirit of nitre, but without any effect; vol. I. p. 75. This

was only spirit of nitre in the common heat of the atmosphere, in a small quantity, and for no long space of time. I therefore considered the experiment as inadequate to the purpose of it; and when I had got my phials with ground-stopples, I introduced the tube of one of them, into which I had put some strong spirit of nitre, under the edge of a small jar, filled with air that had been injured by putrefaction about a year before; and making it boil, forced the hot fumes to rise into, and mix with it, for a considerable time, till the acid seemed to be almost expelled from it; but I could not perceive that the air was sensibly altered by this operation. It was no more diminished by nitrous air than it had been before.

It was on the very same day on which I made the preceding experiment, that it occurred to me that I had, at that very time, a very good opportunity of ascertaining the effect of the fumes of spirit of nitre on common air, by means of a quantity of strong smoking spirit of nitre, made at the Apothecary's-Hall, contained in a large phial, one-fourth part of which was full, and which had not been opened for half a year; so that all the inclosed air, which was three-fourths of the contents of the phial, had been exposed to the vapour of the spirit of nitre all that time.

By losing the spirit of nitre, I might have transferred this air into another vessel, without any mixture of common air; but not chusing to do that, I poured it into another phial, by which means I got a mixture, three-fourths of which was the air from the phial, and one-fourth atmospherical air, for which I had to make an allowance. Examining this mixture by the test of nitrous air, I found that two measures of it, and one of nitrous air, occupied the space of $2\frac{1}{2}$ measures, and a candle would not burn in it; so that the spirit of nitre must have imparted phlogiston to the air which had been exposed to it, and in so great a degree, that it had become almost perfectly noxious; as may be easily concluded, by allowing for the mixture of common and wholesome air with it in this experiment.

That *acid* fumes, as such, have not this effect upon common air, I, at the same time, ascertained, by making the same experiment on the air of the phial which had contained the strongest spirit of salt; and, I believe, for a longer time. This, however, was in all respects as good as common air. This spirit of salt was procured from the Apothecary's-Hall, and smoked very much.

Afterwards,

Afterwards, when I had contrived to fumigate different kinds of air, with the vapour of spirit of nitre, by a singular kind of process, which will be mentioned below, I found that it had no effect whatever upon common air; for in this case, I believe, it contained very little phlogiston: but the experiment was not tried to the greatest advantage.

I was not much more disappointed in my expectations from this experiment than I was in finding that air was injured by being exposed to fresh-melted nitre. I had been led to make this experiment by observing that nitre, when it is fused by heat, yields air. Seeing this, I had the curiosity to try whether it would recover the air it had lost by being exposed to the common air, and at the same time to observe, what effect this exposure would have upon the common air, in order to judge what it was that nitre, in those circumstances, took from the air. And I find that common air, exposed to nitre in these circumstances, is a little injured, but with such circumstances attending the *proof* of it, as I had never observed before, and which I cannot well account for. The facts were as follow.

I melted about an ounce of salt-petre in a crucible, till all the air seemed to be expelled

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from it, and immediately placed it under a receiver standing in water, where it presently became solid. The next morning I examined the air in which it had stood, and found it to be not so good as common air. It was diminished about $\frac{1}{20}$ less than an equal quantity of common air, which I tried at the same time, and with the same nitrous air. I repeated the experiment several times, with the same result.

It was remarkable, however, that after these mixtures had remained a day and a night, they approached nearer to an equality. This also I observed more than once, and am much surprized at the fact. It should seem that the air to which this nitre had been exposed was not absolutely so much injured as the first mixture of nitrous air with it would show; but that its constitution was so much altered, that it required more time for the phlogiston discharged from the nitrous air to act upon it.

Afterwards I melted some salt-petre in a glass-phial, and the vessel being broken by the expansion of the nitre in cooling, I exposed the nitre to a quantity of air confined by water, so that the common air had access to it on all sides; whereas, in the former experiment, it had been contiguous to it at its surface only. After about a week I examined this air, and
immediately

immediately after by the trial with nitrous air, found it to be considerably worse than common air; two measures of it, and one of nitrous air, occupying the space of two measures only: whereas the mixture of common air made at the same time, and with part of the same quantity of nitrous air, was diminished as much as usual. I did not carry this experiment any farther, as I did with another quantity of common air, which had likewise been exposed about a week to melted salt-petre in the same circumstances.

Two measures of this air, and one of nitrous, at first occupied the space of little more than two measures; but it kept continually approaching to the degree of diminution of a mixture of common air made at the same time, till after four days the difference between them was very small. Whether after a longer time these two mixtures would have been reduced to the very same dimensions, I cannot tell. I made no more experiments of this kind, nor have I, in any other respect, pursued this singular kind of fact any farther.

In my former publications, I said I had no doubt but that the nitrous acid might be exhibited in the form of air, and that experiments might be made upon it with a great

prospect of making considerable discoveries, provided that any fluid substance could be found capable of confining it. I have since made several attempts to divest this acid of the water with which it is generally combined; but though I have been favoured by some unexpected circumstances, I have been far from succeeding to my wish.

That this acid is capable of existing in this dry form, I presently satisfied myself by an attempt to expel air from it, by the same process by which I had before expelled the marine acid air, from spirit of salt; viz. by heating the fluid in a phial, and receiving the air in quicksilver. For though the acid vapour very soon united with the quicksilver, yet the jar in which it was received being narrow, the saline crust, which was formed on the surface of the quicksilver, impeded the action of the acid upon it, till I had an opportunity of admitting water to the air that I had produced, and of satisfying myself, by its absorption, of its being a real *acid air*, having an affinity with water, similar to other acid airs.

In the first experiment that I made of this kind, the redness of the air did not appear immediately; but after some time, when it might be presumed that the nitrous vapour had
produced

produced nitrous air, by a solution of the quicksilver; and the redness, I suppose to have been the effect of the mixture of this newly-generated nitrous air, with that portion of common air, which had been contained in the upper part of the phial, and which had been expelled by the acid vapour. I did not admit water to this air till after an hour; and even then it was sensibly diminished; some of the acid air not having been seized by the quicksilver. The last time that I made this experiment, in which I produced about two ounce-measures of air, I admitted water to it as quickly as I could, and then one-third of the whole was imbibed by it.

In my account of the process to procure dephlogisticated air from calcined flint, and also from talck, I have observed that between the produce of the phlogisticated and dephlogisticated air, there is a considerable interval, in which nothing comes over but the pure *vapour of the acid*, which is instantly and wholly imbibed by water. This circumstance gave me a fine and unexpected opportunity of making some experiments upon this vapour. For the orifice of the tube through which it was transmitted being plunged in water, and bending considerably upwards, I could easily put over it phials filled with any kind of air that could bear to be confined by water; and the end of
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the tube rising a considerable way within the phial, the vapour must necessarily come into immediate contact with the air contained in it.

The first experiment that I made upon this vapour, in these circumstances, was with *nitrous air*; and it appeared to have the same effect upon it that had been produced by liver of sulphur, viz. diminishing it till it was no more capable of affecting common air; and the operation was exceedingly quick. Indeed the whole progress of this experiment is not a little remarkable. The moment that the phial of nitrous air was exposed to this vapour, it became white, then transparent, then red; and, lastly, transparent again. I took one quantity of this air, when the whiteness had just gone off; and found that it was but little different from pure nitrous air, diminishing common air almost as much. Taking another phial when it was quite red, one-third of the quantity had disappeared, and its power of diminishing common air was about one-half of what it had been. I then let another phial remain exposed to this vapour, till I perceived that the diminution would go no farther; when only $\frac{1}{20}$ of the original quantity remained, and this did not affect common air at all.

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When this process is quick, that is, when the nitrous vapour comes very fast, the whiteness preceding the redness, on mixing the nitrous vapour with the nitrous air, can hardly be perceived, and the vessel containing the air becomes exceedingly hot, as well as the tube through which it is transmitted. I observed that the vessel containing nitrous air continued exceedingly red for about a minute, without any visible change of dimensions in the air; after which it was suddenly diminished to about one-fourth of its original quantity, which resembles the process of the effervescence of iron-filings and brimstone, described vol. I. p. 118.

I exposed to this nitrous acid vapour, *common air*, *inflammable air*, and *fixed air*, and all of them for a considerable time, without making the least sensible alteration in any of them. It is possible that a longer continuance of the process might have affected them; but a great deal less time was abundantly sufficient for this acid vapour to produce its utmost effect upon nitrous air. It should seem, therefore, that though this acid vapour contained phlogiston enough to phlogistificate, presently and completely, a quantity of nitrous air, it does not contain enough to phlogistificate common
air,

air, at least, that it requires either more time to effect this purpose, or a different mode of application.

As phlogiston had produced no effect upon fixed air, except in one particular case, viz. from the effervescence of iron-filings and brimstone, I did not absolutely expect that it would have been affected in these circumstances. Besides, I only exposed the fixed air to this vapour as it was expelled from the phial by the flame of a candle, when the vapour is not so copious as when it is expelled by a strong sand-heat, surrounding the whole phial placed in a crucible.

In the course of the experiments, I thought I saw reason to conclude that the nitrous acid air is naturally *colourless*, like the other acid airs. For I observed that, though the inside of the phial, and also of the tube, was very red, during the transmission of both the phlogisticated and dephlogisticated air, yet that in the intermediate state, when the pure acid came over, all the inside of the phial was transparent; or if there was any sensible colour, it was of a whitish cast. At the same time it was observable, that this acid vapour, mixing with any other kind of air, produced a red colour.

As

As there was this redness in inflammable air, and other kinds of air, for some time after this vapour was admitted to them, and they afterwards became transparent, I expected that some alteration would have been made in them; but I was disappointed.

I would here observe, that the young operator ought to be very cautious in conducting this process, and especially to take care that the tube through which this acid vapour is transmitted be sufficiently wide; by which I mean that the hollow part of it should be about one-tenth, or one-twelfth of an inch in diameter. When, at one time, I was so incautious as to make use of a tube much smaller than this, almost capillary, some particles of the flint, as I suppose, got into it, and stopped it up. However, there was a violent explosion of the phial, and of all its contents, by which I was exposed to some danger: but providentially, at this time, as upon many other occasions, I escaped without any hurt. But, in such a kind of business as this, nothing can be expected to be done without such risks.

In my former publication, p. 126, I have observed that I got little or no air by dissolv-
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ing *lead* in spirit of nitre. I have since, however, made another attempt of this kind, and with a little better success. I poured smoking spirit of nitre into a phial with a ground-stopple and tube, fig. *c*, containing $1\frac{1}{2}$ ounce-measures, filled with small leaden shot, so as to leave no common air at all, either in the phial or in the tube; and I placed it so as to receive the air that might come from it in water.

After waiting an hour, in which little or no air was produced, I applied the flame of a candle, though not very near to it, and in these circumstances I got about an ounce-measure of air: but upon some water rushing into the phial, while the candle was withdrawn, air was produced very plentifully. I collected, in all, about a quarter of a pint, and might probably have got much more; but that the salt formed by the solution of the lead had so nearly closed up the tube, that I thought proper to discontinue the process. The air, both of the first and of the last produce, was of the same quality, and so far nitrous, that two measures of common air, and one of this, occupied the space of two measures only; excepting that the very first and very last produce, mixed with common air, took up a little more room than
that

that which I got in the middle of the process. When the air was produced very fast, it was exceedingly turbid, as if it had been filled with a white powder.

In my former publication, the reader will have seen the result of several processes in which nitrous air was phlogisticated with iron and liver of sulphur, in consequence of which a candle would burn in it, either naturally, as in common air, or with a beautifully enlarged flame. As this air, in some respects, resembles common air, though it be noxious, it occurred to me, that it might be possible, by means of some ingredients, to make it in all respects common air; and with this, and other views, I, at different times, filled several phials with nitrous air, putting to it iron or liver of sulphur to phlogisticate it, and also pieces of chalk, or a mixture of fixed air, in order to supply it with that ingredient, which it is well known the atmosphere contains; and in other respects also I varied these preparations, in order to have the greater chance of succeeding with respect to the object of the experiment. These projects, the reader will easily imagine, were antecedent to my discovery of the real constitution of the atmosphere, as explained in a preceding section. However, as the processes took
up

up a good deal of time, and some persons, intent upon these pursuits, may wish to know what was the result of them, unsuccessful as they were with respect to my main object, I shall here recite the particulars of the principal of them, in the order in which they were made.

The reader may also have observed, that, in one particular case, mentioned vol I. p. 220, a quantity of nitrous air, which had been exposed two months to some iron nails in quicksilver, was diminished by a mixture of fresh nitrous air. This I find in my register, written at the time of observation, and therefore can hardly doubt but that I must have observed that appearance, which is an indication of a considerable degree of purity in the air, and of its fitness for respiration. But as in none of the following experiments I could get the same appearance, I suspect that I must, some way or other, have deceived myself on the former occasion. It must be observed, however, that it by no means follows, that because we cannot, in a course of experiments, produce the same appearance in what we *imagine* to be the same circumstances, that we were therefore deceived with respect to the appearance itself; because nothing is more common than for persons to be deceived with respect to what they
imagine

imagine to be the same circumstances in an experiment.

June 4, 1774. Two quantities of nitrous air, which had stood above four months in contact with iron in water, just extinguished a candle.

July 25. A candle burned with an enlarged flame; but not more than double, in nitrous air, which had been in contact with iron in quicksilver, about six months. The appearance was the same when the candle was dipped into it, both before it was once passed through the water, and afterwards. Water being admitted to the remainder of this air, it began to be absorbed as usual.

March 2, 1775. Nitrous air, which had been confined above a year in contact with iron, standing in water, was, in all respects, like phlogisticated common air: it neither diminished common air, nor was diminished by nitrous air, and extinguished a candle. It had also the faint smell of phlogisticated air. The more rusty iron is, the faster it diminishes nitrous air, which looks as if it took phlogiston from the nitrous air, rather than communicated any to it.

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March

March 4. In about one day, and without any heat, about one-third of a given quantity of nitrous air was imbibed by liver of sulphur. In the remainder a candle burned with an enlarged flame; but it was not at all diminished by fresh nitrous air.

March 6. Nitrous air, exposed to liver of sulphur and chalk, exhibited the same phenomena as if no chalk had been put to it: it admitted a candle to burn in it with an enlarged flame, was not diminished by nitrous air, and extinguished a candle, after a very little agitation in water.

March 10. A quantity of one-half nitrous, and one-half fixed air, which had been in contact with iron, was reduced one-third in its dimensions, and the remainder admitted a candle to burn in it with an enlarged flame, but was not diminished by nitrous air.

May 7. I examined several quantities of nitrous air, and mixtures of nitrous and fixed air, which had stood exposed in quicksilver, to iron, or to iron that had rusted in nitrous air, about two months. None of them were diminished by nitrous air, or diminished common air. In general they extinguished a candle;

dle; but, in one of them, a candle burned naturally when the fixed air had been washed out of it in water. One quantity of nitrous air, which had been exposed to iron that had rusted in nitrous air, was diminished about one-tenth, but was very little changed; for it diminished common air almost as much as fresh-made nitrous air. Another quantity of nitrous air, which had been exposed to iron-nails, diminished common air rather less than the preceding.

SECTION X.

Some Observations on COMMON AIR.

It is generally supposed, and perhaps with reason, that the use of some metals is much safer than others, on account of some *effluvia* issuing from them. Copper and lead, for instance, are thought to have some noxious quality of this kind; whereas iron is thought to be perfectly harmless, in every shape, as Dr. Franklin humorously observes, excepting in that of a weapon. My experiments on the diminution of air, by paint, consisting of white lead and oil, by which it is rendered perfectly noxious, have been interpreted so as to favour that opinion; and, in my account of those experiments, I had myself ascribed the effect to the phlogiston remaining in the white lead.

I had then, however, observed, that air is as much diminished, and consequently is rendered as completely noxious, by the calcination of *tin*, as by that of lead; and tin, I believe, does not lie under any peculiar odium.

Indeed, if my hypothesis be well founded (and so many facts have occurred to my observation in support of it, that I do not see how it can be called in question) viz. that it is phlogiston that diminishes air, and renders it noxious; it must be a matter of indifference from which of the metals it is discharged, and the calcination of any of them must render the air in which the calcination is made equally noxious. Other facts have occurred in the course of my late observations that farther confirm this opinion.

When I was making the experiments on the extraction of inflammable air from iron, I found, that if the quantity of air was considerable, the throwing of the focus of the burning lens upon iron-filings inclosed in it, had no other effect than to diminish the air, and make it noxious; which it did, to as great a degree as the calcination of lead or tin had done: for after this it made no effervescence, and was no farther diminished, by nitrous air. After this, I make no doubt, but that if the process had been continued a sufficient time, there would have been an increase of the quantity of air, by the production of inflammable air; but the first effect of the discharge of phlogiston from the iron was the phlogisticating and diminishing of the common air.

I even found that air would be injured by having iron confined in it for a considerable time. To try this, I filled a phial, containing common air, full of nails, the 18th of December, 1773, and left it inverted in a vessel of water till the 2d of March, 1775, when I found that it was diminished one-fifth of its bulk, and was not in the least diminished by a mixture of nitrous air; so that it must have been perfectly noxious.

I have also seen reason to think, that my former opinion concerning the cause of the diminution of air by paint, viz. that it was effected by the discharge of phlogiston from the *white lead*, was not well founded. For in making my experiments with *red lead*, having found before, that a composition of which turpentine made a part, had diminished common air (vol. I. p. 179) and therefore, suspecting that possibly it might be the oil of turpentine, and not the white lead, that contributed most to the diminution of the air, I got a small quantity of *paint made with red lead*, mixed in the usual manner; and found, that when I had daubed pieces of paper with this paint, and covered them with a jar standing in water, the air was diminished just as before; so that it was probably the phlogistic effluvia
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of the *oils*, and not of the *lead*, that produced this effect.

In my former publication, I observed that air, which had been exposed to the effluvia of the common *red cement*, was injured by it. For this purpose, I had covered all the inside of a phial with it, and placed it, inverted, in a vessel of water. Since that time I have repeated, or rather continued the experiment, letting the same vessel stand about nine months in that situation; when, upon examining it, I found it was diminished one-fifth of its bulk, and that it was not at all affected by nitrous air.

Having had so much to do with *red lead*, in the course of my late experiments, I had the curiosity to try what would be the effect of that process by which the *grey calx of lead* is converted into red lead. The simple calcination of lead, I knew to be the discharge of phlogiston, by which the air contiguous to it is diminished; and, as far as I have had an opportunity of observing, the continuance of the same process, by which the grey calx is converted into red lead, is of the same nature, viz. a farther discharge of phlogiston, and consequently a diminution of the air. For I threw the focus of a burning lens upon a small quantity

of the grey calx of lead, placed under a receiver standing in water; and though this operation did not make it absolutely red lead, it gave it a reddish tinge; and I found, by the test of nitrous air, that the air in which the experiment was performed was considerably injured, being not nearly so much affected by nitrous air as common air is.

As common air contains a considerable proportion of fixed air, I was desirous, from the time that I began my experiments on air, to extract the fixed air from a given quantity of atmospherical air, to see what it would be when deprived of that ingredient. I had found, indeed, that fixed air is precipitated from common air in phlogistic processes, and especially by the electric spark; but this is a complex process; for at the same time that fixed air is discharged, phlogiston is received by the air. But nothing that I have yet thought of has succeeded simply to extract the fixed air.

As quicklime has a great affinity with fixed air, I thought it possible that confining a large quantity of quicklime in a small quantity of air might, in time, produce some effect of this kind; but the experiment did not answer, though I crowded a great number of small pieces of the best quicklime that I could procure,
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into a phial, which I left inverted in a basin of quicksilver a whole week. It did not appear that the inclosed air was at all sensibly affected. Had it been so in the smallest degree, I should have repeated the process, and have allowed it more time.

Having discovered that vegetation restores, to a considerable degree of purity, air that had been injured by respiration or putrefaction; and also that agitation in water produces the same effect, I conjectured that the phlogistic matter, absorbed by the water, might be imbibed by plants, as well as form other combinations with substances under the water. A curious fact, which has since been communicated to me, very much favours this supposition.

Mr. Garrick was so obliging as to give me the first intimation of it, and Mr. Walker, the ingenious author of a late English Dictionary, from whom he received the account, was pleased to take some pains in making farther inquiries into it for my use. He informed me that Mr. Bremner, who keeps a music-shop opposite to Somerset-house, was at Harwich, waiting for the packet; and observed that a reservoir at the principal inn was very foul on the sides. This made him ask the inn-keeper
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why he did not clean it out; who immediately answered, that he had done so once, but would not any more; for that after cleansing the reservoir, the water which was caught in it grew fetid, and unfit for use; and that it did not recover its sweetness till the sides and bottom of the reservoir grew very foul again. Mr. Walker questioned Mr. Bremner, whether there were any vegetables growing at the sides and bottom of it; but of this he could not be positive. However, as he said it was covered with a *green substance*, which is known to be vegetable matter (and indeed nothing else could well adhere to the *sides*, as well as to the bottom of the reservoir) I think it will be deemed probable, that it was this vegetating matter that preserved the water sweet, imbibing the phlogistic matter that was discharged in its tendency to putrefaction.

I shall be happy, if the mention of this fact should excite an attention to things of this nature. Trifling as they seem to be, they have, in a philosophical view, the greatest dignity and importance; serving to explain some of the most striking phenomena in nature, respecting the general plan and constitution of the system, and the relation that one part of it bears to another.

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SECTION XI.

Of the Fluor Acid Air.

The philosophical part of the world have, of late, been highly gratified by the discovery of what was imagined to be *a new mineral acid*, contained in a substance which the chymists distinguish by the name of *fluor*; but many of my readers will understand me better, when I inform them, that it is of that species of substance, which, with us, is called *the Derbyshire spar*; and of which, at present, vases and other ornaments for chimnies, are usually made. The acid is expelled from this substance by oil of vitriol, and has peculiar properties, as remarkable as any of the other three mineral acids which we were acquainted with before.

This curious discovery was made by Mr. Scheele, a Swede; from which circumstance the acid is often distinguished by the name of the *Swedish acid*. His method of operating upon this substance, and likewise that of all who have succeeded him in the inquiry, was to distil it in glass-vessels, as in the process of making spirit of nitre from salt-petre; and the
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most remarkable facts that have been observed concerning it are, that the vessels in which the distillation is made are apt to be corroded; so that holes will be made quite through them; and that when there is water in the recipient, the surface of it will be covered with a crust, of a friable stony matter.

This crust, which I shall distinguish by the name of *the fluor crust*, Mr. Scheele supposed to be *quartz*; and therefore concluded that this acid and water were the constituent parts of that fossil. On the other hand, Mr. Boulanger, who has taken a great deal of pains with this subject, is of opinion that this new acid is only the *acid of salt*, combined with an earthy substance. For this opinion he advances various reasons; but does not pretend to be able to produce any decisive proof. The result of my own experiments, I think, clearly prove, that the fluor acid is the *acid of vitriol*, charged with so much phlogiston as is necessary to its taking the form of air, and also with much of the earthy matter of the spar.

As soon as I had exhibited one of the acids in the form of *air*, I had no doubt but that all the acids might be exhibited in the same manner, and this among the rest; but I imagined that I should find great difficulty in procuring
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the fossil that contains it; supposing that it had only been found in Sweden; and I should probably have continued in this incapacity for making the following experiments, had I not been relieved by Mr. Woulfe, who, upon my inquiry concerning it, not only explained to me what the substance was, but immediately furnished me with a quantity of several kinds of it, sufficient for my purpose. That with which my first experiments were made, was that which he called *the white phosphoric spar, from Saxony*; but afterwards I made use of the Derbyshire spar; and the pieces that I had by me were partly white, or yellowish, and partly purple.

All my advantage in the investigation of this subject, has arisen from my peculiar manner of conducting the experiments. For, by exhibiting the acid in the form of air, free from all moisture, I had an opportunity of examining its nature and affinities with the greatest ease and certainty. In this manner also, this species of air exhibits a variety of striking phenomena, which cannot be produced in any other manner of operating upon it.

When I began these experiments, I followed the directions given by those who had gone before me in the investigation of this subject, and who had procured the acid in the common method

thod of distillation, pounding the fluor (which I afterwards found not to be necessary) and pouring oil of vitriol upon it. This I did in a phial, to which was fitted a ground-stopper and tube, and immediately found, that, at first, without any heat, and afterwards with a very small degree of it, air was produced in great plenty, perfectly transparent, and confined by quicksilver, like the other acid airs. The vapour, as it issued out of the tube into the open air, formed a permanent white cloud; no doubt, by attaching to itself the water that floated in the atmosphere, and the smell of it was extremely pungent.

I had no sooner produced this new kind of air, but I was eager to see the effect it would have on *water*, and to produce the stony crust formed by their union, as described by Mr. Scheele; and I was not disappointed in my expectations. The moment the water came into contact with this air, the surface of it became white and opaque, by a *stony film*, which, forming a separation between the air above, and the water below it, considerably retarded the ascent of the water, till the air, insinuating itself through the pores and cracks of this crust, the water necessarily rose as the air diminished, and breaking the crust, presented a new surface of water, which, like the former,
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was instantly covered with a fresh crust. Thus was one stony incrustation formed after another, till every particle of the air was united to the water, and the different films being collected, and dried, formed a white powdery substance, generally a little acid to the taste; but when washed in much pure water, was perfectly insipid.

Few philosophical experiments exhibit a more pleasing appearance than this, which can only be made, by first producing the air confined by quicksilver, and then admitting a large body of water to it. Most persons to whom I have shewn the experiment have been exceedingly struck with it. It is exhibited to the most advantage, when the vessel that contains the air is pretty wide, by which I mean about an inch in diameter. In this case the crust will often crack in the middle, and a small jet of water rushing through the fissure, will, to appearance, be instantly converted into this stony substance, and look like a puff of white powder, rising sometimes an inch or two up into the air. Also the *crystallizations*, formed on the sides of the vessel, as the water rises in it, make a very beautiful appearance.

The union of this acid air and water may also be exhibited in another manner, which,

to some persons, makes a still more striking experiment; viz. by admitting the air, as fast as it is generated, to a large body of water resting on quicksilver, instead of introducing the water to the air previously formed.

For this purpose, I usually put two or three ounce-measures of water into a tall cylindrical jar, about an inch in diameter (such as those which I generally use as recipients of those kinds of air that must be confined by quicksilver) and filling the remainder of the vessel with quicksilver, I place it inverted in a basin containing a quantity of the same fluid; so that the water, immediately rising to the top, occupies the upper part of the vessel, while the quicksilver occupies the lower part. I then introduce under it the end of the tube proceeding from the phial, which contains the materials for generating this air. It is, then, very pleasing to observe, that the moment any bubble of air, after passing through the quicksilver, reaches the water, it is instantly, as it were, converted into a stone; but continuing hollow for a short space of time, generally rises to the top of the water, in the form of a bubble, or a thin white film. If the succession of bubbles be rapid, and they rise freely to the top of the vessel, through a large body of clear water (which, however, is not always the case,

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as they will sometimes adhere to the upper surface of the quicksilver) I have met with few persons who are soon weary of looking at it; and some could sit by it almost a whole hour, and be agreeably amused all the time.

Every bubble of air, coming into contact with the water on every side at once, is like a bladder, being hollow within; but this slight crust soon bursting, the sides collapse, and it rises to the top of the vessel, in the form of a piece of thin white gauze; but the water soon penetrating every part of it, the whole mass of these films becomes in a little time like a jelly, which continually thickens by the accession of more films, till at length the whole body of water seems to become solid; so that, being fully saturated, especially at the lower part, the air, finding no more moisture within its reach, will fill all the lower part of the vessel, expelling the quicksilver, while the water, in the form of a stiff jelly, occupies all the upper part of the vessel.

As, for the purposes to be mentioned hereafter, I have repeated this experiment a great number of times, I have had an opportunity of observing a very great variety in the appearances which it exhibits. One is peculiarly pleasing, but not very common. A large
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bubble of air will sometimes adhere, by its lower part, to the surface of the quicksilver; and another bubble, rising in the same place, before the lower part of the former has been closed, pushes out the upper part of it, and advancing farther into the water, extends the bubble in length: another follows, and does the same, till at length a *tube* is formed (the sides also growing thicker continually) extending from the quicksilver, to the top of the water. I have seen of them four inches in length; and others being formed close to them, the whole vessel has been almost filled with these tubes, adhering to one another, of different lengths, and not much unlike the appearance of the pipes that are placed in the front of an organ.

In less than an hour, I have frequently converted two or three ounce-measures of water into this solid mass. When this is taken out of the vessel, and pressed, it will be found to contain a great deal of an acid liquor; the water impregnated with the acid having been intangled in the interstices of the jelly, out of the reach of the air: and if this liquor be used in another process, instead of pure water, more of it will seem to become solid, and the acid liquor will be concentrated every time.

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By the repetition of this process, an acid liquor may be procured of a very considerable degree of strength. There seems, however, to be a limit to its strength; for the acid is exceedingly volatile, as is evident from its extremely pungent smell; so that I have thought that I gained nothing by repeating the process more than eight or ten times; because it was impossible to transfer the water from one vessel to another, but more acid would be lost by evaporation, than would be acquired by another impregnation with the acid air.

These appearances I explain, by supposing that the vitriolic acid, in uniting with the spar, is in part volatilized, by means of some phlogiston contained in it, so as to form a vitriolic acid air; and that there is also combined with this air, a portion of the solid earthy part of the spar, which continues in a state of solution, till, coming into contact with the water, the fluid unites with the acid, and the earth is precipitated. The reasons on which I found this opinion will appear in due time; but to make my reader follow me, step by step, in my analytical progress, I must first acquaint him with the observations I made upon this acid air, in its compound state, before the stony matter was separated from it. For this union makes it quite another thing, giving it

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peculiar properties, which are not to be discovered in the pure acid air, divested of that stony matter; and therefore, though it be a compound, and may be analyzed into its constituent parts, it is sufficiently intitled to a peculiar appellation, viz. that of *the Fluor acid air*.

Before I proceed to relate any of the experiments which I made with this acid air, I shall give a few directions and precautions, which may be useful to persons on their first entrance upon this course.

1. The tube through which this acid vapour is conveyed should not be very narrow, because it is apt to be furred up, especially when any phial, containing materials for the production of this air, has been used some time, and with a good deal of heat; owing, I suppose, to the hot air retaining in solution more of the stony matter than it can do when it is cold, and therefore depositing it as it is conveyed through the tube.

2. I began these experiments with phials which had ground-stopples and tubes, but soon found that it was too expensive a mode of experimenting with this kind of air; for they were presently corroded and spoiled. Afterwards,

terwards, therefore, I used only common phials, but the thickest that I could meet with; and still seldom found that they would bear the experiment above an hour. Very frequently, the thickest phials that I could get would be worn quite through in a quarter of an hour, when the heat was considerable, and the production of the air rapid. This power of dissolving glass is a very remarkable property of this air; but it seems to possess it only when it is hot, at least in any considerable degree.

3. When I wished to produce this air pretty fast, I found it most convenient to pound the spar, and pour the oil of vitriol upon it, filling one-fourth of the phial with the spar, and leaving one-fourth of it for a space in which the bubbles might expand themselves, and break, so as not to carry any of the liquor into the tube. I now proceed to the particular experiments.

Dipping a lighted candle into a vessel filled with the fluor acid air, it was extinguished without any particular colour of the flame, which is observable in the marine acid air.

The mixture of any other of the acid airs, with *alkaline air*, makes so beautiful an experiment, that it was naturally one of the first

experiments I thought of making with this new acid air. Accordingly, I got the appearance that I had expected; a white cloud being formed by the union of these two kinds of air. But the alkaline air did not mix so readily with this as with the other kinds of acid air; and which surprized me much at the time, the salt formed by the union of these two kinds of air was not soluble, either in water or spirit of wine. But, in fact, the proper *salt* formed by the union of these kinds of air was, no doubt, dissolved in the water; that which remained undissolved being, as I conjecture, the *stony substance* only which had been held in solution in the acid air. This stony substance, being mixed with the acid air, is also probably the reason why the alkaline air does not mix so readily with it as with the other kinds of acid air; some time being requisite to disengage it from this stony substance, in order to its uniting with the alkaline air.

Nitrous air, mixed with this acid air, had no sensible effect upon it. Water absorbed the acid air, and left the nitrous air possessed of its peculiar properties.

Having ascertained the effect of water upon this acid air, I proceeded to try other *fluid substances*.

Spirit

Spirit of wine imbibed this air as readily as water, but continued as limpid as ever; and when saturated with it, seemed to be no less inflammable than before.

Oil of turpentine, did not imbibe any of this air.

Vitriolic ether imbibed about twenty times its own bulk of it; but was not sensibly changed by the impregnation. The case was the same with *nitrous ether*. But the first time that I made the experiment with nitrous ether, I imagine a little water was mixed with it (as much as those substances are capable of being mixed) for it coagulated as water had done, remaining in the middle of the tube, the acid air being both above and below it. This mass of coagulated matter; which in colour and consistence resembled a brown jelly, being taken out of the vessel, did not take fire at the approach of a candle; but when it had been exposed to the air about half a minute, it grew hot, threw out a gross smoke, and was presently all evaporated. Part, however, of the same mass, which had been dipped in water, did not grow hot, or evaporate, in the open air; and when exposed to the fire, it burned to a white powdery substance. I imagine this effect to have been owing to a mixture,

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ture of water ; because, with pure nitrous ether, I could not get another appearance of the kind.

Of *solid substances*, I found that this air had no effect upon *brimstone*, *common salt*, *sal ammoniac*, *iron*, *liver of sulphur*, or *gum-lac*.

Charcoal absorbed the whole of a quantity of this air, and contracted from it a strong pungent smell. The *rust of iron* also absorbed it in like manner.

Alum absorbed this air pretty fast, the surface of it being rendered white and opake. When it was taken out of the air, it looked moist, and was incapable of the operation of roasting, like that which had been exposed to alkaline air. This air having, no doubt, like the other, seized upon the water which enters into the composition of alum.

Quicklime and *chalk*, both absorbed a little of this acid air ; but the result was, in no respect, remarkable. The latter had been dissolved by it, and had produced a quantity of fixed air, precipitating lime in lime-water.

In order to judge whether there was any foundation for the opinion of Mr. Boulanger, of this
acid

acid being the same with the *marine*, I put to it a piece of salt-petre, which I have observed to be readily dissolved in the marine acid air; and I must own that appearances so much favoured his opinion, that I was at that time very much inclined to adopt it.

When the salt-petre had been for some time surrounded with this air, the air began to be diminished, and the inside of the vessel was filled with red fumes, which continued about a week, the quicksilver rising all the time, till only one-tenth of the air remained, and the inside of the vessel was covered with a whitish, probably a saline substance, produced by the solution of mercury. After this, the air becoming transparent, I examined it, and found it neither to affect common air, nor to be affected by nitrous air, and to extinguish a candle. Also, about one-fourth of it was readily absorbed by water, and made lime-water turbid; so that, contrary to my expectation, a great part of the air must have been fixed air, and not nitrous. This experiment I did not repeat; but it seems to exhibit a fact deserving particular attention, in the investigation of the nature of fixed air.

I thought it might possibly contribute to decide the question concerning the identity of
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this acid and the marine, if I put a quantity of the *fluor crust* to marine acid air; thinking that they might form an union, and constitute this fluor acid air: and, indeed, something similar to it was by this means produced; so that another crust was formed upon the admission of water to it; but, in other respects, several circumstances, which I cannot explain, attended the experiments. They were as follows.

To about two ounce-measures of marine acid air I put about a quarter of a grain of the fluor crust, and in about three days it had absorbed about half an ounce measure of the air. Water being then admitted to it, left a quarter of an ounce-measure of air unabsorbed.

Afterwards I conveyed marine acid air to a pretty large quantity of the fluor crust, confined by quicksilver; and, as the air was imbibed, I continued to throw up more, till, after three or four days, that substance seemed to be fully saturated with the air. Then admitting water to it, it was absorbed exactly like the fluor acid air: but I could not, at that time, very well distinguish the crust on the top of it, on account of the jar being almost filled with the crust, and part of it floating on the top of the water. About three-fourths of this air

was absorbed by the water ; but what I thought very remarkable, air kept issuing from this fluor-crust, in large bubbles, till the quantity of air was doubled, and the jar was half-filled with it. This air neither affected common air, nor was affected by nitrous air, and it extinguished a candle.

I repeated the experiment, with this only difference, that I admitted water to the air as soon as the fluor-crust seemed to be saturated ; when the experiment being made in a wide jar, the crust on the surface of the water was as visible as in the experiment with the fluor acid air itself. At this time, however, there was no generation of air from the saturated crust, as before, but a considerable quantity of air, unabsorbed by water, though I took care that the marine acid air was as pure as I could procure it.

Having a quantity of the fluor-crust saturated with marine acid air, I had the curiosity to pour some oil of vitriol upon it, in order to try whether the produce would be pure marine acid air, or a mixture of the two ; and the latter seemed to be the case, though I think the marine acid prevailed in the mixture,

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In this process air was produced in great plenty, and the bubbles burst in the receiver with a white cloud; but when water was admitted to it, it was absorbed without any crust being formed upon its surface. In twenty-four hours a piece of salt-petre turned yellow in this air, and absorbed about half an ounce-measure of it. What remained unabsorbed by water, was exceedingly strong nitrous air, the spirit of nitre having been set loose from the salt-petre by the marine acid air, and having dissolved the quicksilver.

A piece of *borax*, in about a fortnight, absorbed about two ounce-measures of this air, without leaving any residuum not absorbed by water. The surface of the borax was become soft; but by washing it in water, the soft part was easily separated from the rest.

At the very beginning of my investigation of this subject, I had a suspicion that this new acid air might possibly be the *vitriolic acid air*, loaded with the sparry crust; but not succeeding in the experiments which I thought must have decided the question, I quitted that hypothesis for some time. The experiments were these.

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I threw the focus of a burning lens upon some pieces of the spar in vitriolic acid air, confined by quicksilver; thinking, that when it was hot, it might dissolve some part of it, and thereby become the same thing with the fluor acid air. But though I continued this operation till the spar smoked, and filled the vessel with a white fume, there was neither any addition made to the quantity of air, nor any change produced in the quality of it. When water was admitted to it, no crust, as I had expected, was formed on the surface of it.

In order to try whether the fluor crust was the same thing with the spar, from which it had been produced, I got a quantity of it, and treated it in the same manner as I had treated the spar, pouring oil of vitriol upon it, and endeavouring to expel air from it. I presently found, indeed, that it yielded great plenty of air; but not finding it to be the thing I was then in quest of, viz. an acid air, by means of which a crust would be formed on the surface of the water admitted to it, I neglected to give sufficient attention to it, or I might have been led to suspect that this fluor-crust, like the fluor itself, contains so much phlogiston, as, by incorporating with the oil of vitriol, to enable it to assume the form of air, and become the vitriolic acid air; though the earthy matter,
not

not incorporating with it, it could not become the fluor acid air.

Water admitted to this acid air, procured from the fluor-crust by oil of vitriol, absorbed it all, but without having any crust upon its surface.

Alkaline air united with the whole of this acid air, forming with it a white saline substance; and part of the inside of the tube in which the mixture was made, was tinged with a deep yellow, or orange-colour, which disappeared after a few hours exposure to the open air. This I have observed to be the case with the vitriolic acid air.

This air did not at all affect *salt-petre* or *borax*.

Had I, however, prosecuted these experiments farther, and have found, as, I doubt not, I should have done, that this acid air, procured by oil of vitriol from the fluor-crust, was genuine vitriolic acid air, it would have proved no more than that this fluor-crust contains phlogiston, and in such a state as to combine with oil of vitriol by heat, and enable it to take the form of air. They would not have proved, that the air procured from the fluor
itself

itself was of that nature: for it might have been said, that the peculiar acid of the fluor had been expelled before.

To make the *experimentum crucis* in this case, I saturated a quantity of water with the fluor acid air, pressing out the stony matter with which it was filled at each process, and impregnating it over and over again. When it appeared to be sufficiently impregnated for my purpose, I put the liquor into a phial, furnished with a proper tube and recipient, such as is represented plate II. fig. 8. vol. I. to receive any of the watery part that might be expelled by heat; and applying the flame of a candle, I presently got from it great plenty of air; which, by every test that I could think of applying, appeared to have the very same properties with the vitriolic acid air, of which an account was given in the first section of this volume.

The air thus expelled from this acid liquor was absorbed by water, without any crust on the surface of it.

When alkaline air was admitted to it, the sides of the vessel were tinged with the orange-colour mentioned above, which vanished in
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about an hour after it had been exposed to the open air.

This air had no effect upon *salt-petre*; a piece of which continued in it about a fortnight; nor yet upon *brimstone*, *alum*, or *sal-ammoniac*.

Liver of sulphur absorbed it, without undergoing any sensible change.

This air extinguished a candle, without any particular colour of the flame.

Camphor was dissolved in this air, exactly as it is in vitriolic acid air.

In these properties this acid air will be found, by comparison, to agree with the vitriolic acid air; as also in the two following, which, as far as I know, are peculiar to this species of air.

Phlogiston, as I have observed, is contained in vitriolic acid air, and in such a manner as to be communicated by it to the common air with which it is mixed, and thereby to phlogistificate or injure it. And an equal quantity of this acid air, and common air, having been mixed, and left together twenty-four hours, the common air appeared to be so far injured,
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that two measures of it, and one of nitrous air, occupied the space of something more than two measures.

The *electric spark* has a very remarkable effect upon the vitriolic acid air; or rather upon the glass-tube in which the experiment is made, as will be particularly noted hereafter; for a single explosion covers all the inside surface with a deep brown or black matter, and the glass grows more opaque every stroke. This very singular and striking effect has the electric spark taken in the air expelled from this acid liquor.

After I had made this experiment, I had no doubt, but that these two kinds of air, viz. the *vitriolic acid* and the *fluor acid*, are, in reality, the same. It is possible, however, that there may be some small difference between them, in consequence of the air from this acid liquor still containing some portion of the earth of the spar. I conjecture that it does; because, towards the end of the experiment, when the liquor was made to boil with violence; the inside of the tube immediately connected with it, was filled with a stony matter. It happened twice in the course of the above mentioned experiments, that the tube was quite stopped up
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by this means, so as to cause the explosion of the phial.

Lastly, I would observe, that the *taste* of this acid liquor affords a strong presumption, that the acid which enters into it is the vitriolic; for it has exactly the astringency of alum.

That the fluor contains phlogiston, is evident, from the attempts that I made to procure dephlogisticated air from it, by means of spirit of nitre; for the air that I got from it was always phlogisticated, and sometimes even nitrous.

At first I made this experiment by putting the materials into a phial with a ground-stopper and tube, and applying the heat of a candle only. The air I got in this manner neither affected common air, nor was affected by nitrous air. I then put the same apparatus into a crucible; and, with a strong sand-heat, I got from it about two ounce-measures of air, in four portions. The first of these was exactly like the preceding, being phlogisticated air; the second made lime-water turbid, and a great part of it was readily absorbed by water: the third and fourth portions were very strong nitrous air.

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This experiment was made with the whitish part of this spar, which therefore probably contains the least phlogiston. That phlogiston which contributes to the *colour* of this fossil, I found, by the following observation, to be of a very volatile nature. When the coloured spar is dissolved in oil of vitriol, the fluor-crust, collected in the water, has the same colour; but when it is dried near the fire, the colour vanishes, and the whole becomes white: yet this white crust, heated again in oil of vitriol, contains, as was observed before, so much phlogiston, as to convert oil of vitriol into vitriolic acid air.

The air expelled from this acid liquor did not dissolve the *fluor-crust* that was exposed to it. A quantity of it remained in this situation several days, without affecting it, or being affected by it. I had imagined that it might have been dissolved by this air, and have converted it into the fluor acid air.

Oil of turpentine absorbed about ten times its bulk of this air, and became of an orange-colour. After this impregnation it had a pungent acid smell, together with its own. I observed nothing farther respecting it.

Reflecting upon the phosphoric property of the spar, by means of which I had procured this acid air, I thought it was possible that its property of enabling oil of vitriol to yield this air, might be common to it with other similar phosphoric substances, depending upon that combination of phlogiston which enables them to imbibe and emit light.

In order to ascertain this, with respect to one other substance of this kind, I made a quantity of Mr. *Canton's phosphorus*, and pouring upon it some oil of vitriol, I got air that was readily absorbed by water, and with a crust upon its surface, exactly like that which is procured from the fluor, only not in so great a quantity. The effervescence between this substance and the oil of vitriol was very great, and also the heat occasioned by it; and the vapour escaping into the common air, was white and dense, much like the vapour of the fluor acid.

I shall conclude this section with observing, that the oil of vitriol in which the fluor is dissolved, becomes thick, like ice, exactly like the oil of vitriol in which quicklime has been boiled, as will be particularly noticed hereafter.

SECTION XII.

Experiments and Observations relating to FIXED AIR.

Fixed air was the first species of air that was discovered, distinct from common air; much has been done towards the investigation of its properties, and several capital uses have been made of it. There is still, however, great difference of opinion among philosophers concerning it; and, in a variety of respects, much remains to be done towards completing our knowledge of it, and especially of its relation to the other acids with which we are acquainted. I cannot say that I have, of late, given much attention to this kind of air; but several things have occurred to me in the course of the experiments already recited, and others, which tend to throw a little light upon the subject; and a few more observations and experiments, not connected with them, I have reserved for this separate section.

Having been informed by a correspondent in Italy, that air, expelled from lime-stone,
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by means of heat, would not acidulate water ; from which he concluded that its acidity, and even its substance, was derived from the oil of vitriol employed in the production of it: I filled a tall glass-vessel, represented fig. *d*, with powdered chalk, and with a strong sand-heat, expelled from it a considerable quantity of air, which appeared to me to be absorbed by water, exactly like fixed air; the usual proportion only remaining unabsorbed. It also precipitated lime in lime-water ; so that without impregnating water with it, so as to taste of it, I entertained no doubt of its being genuine fixed air, and having all the properties of the air that is expelled from chalk by oil of vitriol. Mr. Bewley, as the reader will see in my Appendix, also found that fixed air, procured by means of heat only, changed the blue colour of water (tinged with the juice of turnsole) to red.

Air from wood and charcoal, also, is undoubtedly fixed air, though no acid be employed to expel it, and it be mixed with inflammable air. I received air, expelled by heat, from two ounce-measures of charcoal in a tall glass-vessel, fig. *d*, in three parts, each containing about a pint; and observed, that in every part of the process the air made lime-water turbid. But there was more fixed air in
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the first portion than afterwards; for about one-fourth only of the first portion remained unabsorbed by water; but of the second and third portions one-half nearly was unabsorbed. The residuum was inflammable.

When heat can expel no more fixed air from charcoal, it should seem that spirit of nitre (if this acid itself be not converted into fixed air) can extract more from it. For when I dissolved, in spirit of nitre, some pieces of charcoal, which had been made with the strongest heat of a smith's fire, long continued, so that no more air could be expelled from them by that means; part of it was evidently fixed air, as appeared by its precipitating lime in lime-water.

There are few substances in nature that do not contain fixed air, discoverable either by heat, or by some stronger acid. In general, acids will detect fixed air more readily than heat; but this is not the case with respect to *clay*, except when it is strongly heated in spirit of nitre; for it makes no effervescence upon being mixed with any of the acids: but a degree of heat sufficient to bake the clay, evidently expels fixed air from it. In order to ascertain this fact, I filled a gun-barrel with tobacco-pipe clay, and, putting it into the

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fire,

fire, I received the air that came from it, in several portions; but the whole was not more than about five times the bulk of the clay. The first produce was inflammable; but afterwards the air was fixed, precipitating lime in lime-water, and being readily absorbed by water. I never met with purer fixed air; but I had no suspicion of this at the time that I procured dephlogisticated air, mixed with fixed air, from clay made into a paste with spirit of nitre.

It might be questioned, whether the fixed air contained in our aliments, can be conveyed by the course of circulation into the blood, and by that means impregnate the urine. I have found, however, that it may do it; having more than once expelled from a quantity of fresh-made urine, by means of heat, about one-fifth of its bulk of pure fixed air, as appeared by its precipitating lime in lime-water, and being almost wholly absorbed by water; and yet a very good air-pump did not discover that it contained any air at all.

It must be observed, however, that it required several hours to expel this air by heat; and after the process, there was a considerable whitish sediment at the bottom of the vessel. This was, probably, some calcareous matter
with

with which the fixed air had been united ; and by this fixed air, the calcareous matter, which would otherwise have formed a stone or gravel, may have been held in solution ; and therefore, drinking water impregnated with fixed air, may, by impregnating the urine, enable it to dissolve calcareous matters better than it would otherwise have done, and may therefore be a means of preventing or dissolving the stone in the bladder, agreeable to the proposal of my friend Dr. Percival ; for which see the Appendix.

That fixed air is always contained in common air, is evident from many observations, and especially from the precipitation of it by means of nitrous air, the electric spark, and other phlogistic processes. It is likewise contained in the purest dephlogisticated air, as appears by mixing nitrous air with it in lime-water, which is thereby rendered slightly turbid. It has also been seen, that when dephlogisticated air is first procured, by any process whatever, there is always a considerable quantity of fixed air mixed with it. There is the least when it is got from *mercurius calcinatus per se* ; but I have always found some when the air was expelled from this substance by a burning lens, either in quicksilver, or *in vacuo*.

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In the former volume I have said, that when nitrous air is mixed with common air that had been injured by some phlogistic process, and restored by agitation in water, there was no precipitation of fixed air; but this must have been a mistake: for I have since repeated that experiment with the greatest care, and find a contrary result; and I have used every precaution that I could think of, in order to guard against a mistake in the process; particularly, lest the air upon which I was operating should receive a mixture of any other kind of air from the water in which it was agitated, I previously boiled the water for several hours, in order to expel all its own air from it.

Having done this, I observed that, immediately after the restoration of the noxious air by agitation in this water, it always made lime-water slightly turbid; but this was not the case, after passing two or three times through the lime-water. When it was, by this means, entirely purged of fixed air, I admitted nitrous air to it in lime-water, and there was a very evident precipitation of lime, quite as much as when nitrous air is mixed with common air that has not been injured at all.

It is not easy to say whence this fixed air could come. If all the fixed air had been discharged

charged by the first phlogistic process, that which appeared in the second must either have come from the water, though it had been boiled, which I do not think probable, or from the nitrous air, which, though it be inexplicable, is perhaps less improbable upon the whole.

Mr. Cavendish observed, that a certain portion of fixed air is no more liable to be absorbed by water than common air. This, he states at about one-sixtieth part of the whole. I had the curiosity to try, whether, if I saturated a quantity of water with fixed air, and expelled it again by heat, that very air which had actually been in the water, would not be wholly imbibed by fresh water; and whether I could not, by this means, get a purer kind of fixed air than that which is immediately procured by means of chalk and oil of vitriol. This experiment I made twice, with all the care that I could apply, and found, in both the cases, that even the fixed air which had been in the water, contained as large a portion of that which would not be imbibed by water again, as the air which had been immediately dislodged from chalk by oil of vitriol.

In order to be more sure of this fact, I was more especially careful, the second time that I made the experiment, to use every precaution
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that I could think of, in order to prevent any error in the conclusion. For this purpose, I took rain-water, and boiled it about two hours, in order to get it perfectly free from air; and I began to impregnate it with fixed air a long time before it was cold, and therefore before it could have imbibed any common air; and, in order to expel the air from it, I put it into a phial, which I plunged in a vessel of water set on the fire to boil, taking care that both the phial containing the impregnated water, and the glass-tube through which the air was to be transmitted, were completely filled with the water, and no visible particle of common air lodged on the surface of it. I also received the expelled air in water, which contained very little air of any kind, lest the very small degree of agitation which I made use of, in order to make the water re-imbibe the air, should disengage any air from it. Also, that less agitation, and less time, might be sufficient, I chiefly made use of lime-water for this purpose. But notwithstanding all these precautions, I found a very considerable residuum of air, not less than Mr. Cavendish had stated, that water would not imbibe.

At a time when this residuum of fixed air hardly gave the least sensible whiteness to lime-water, I examined the state of it, and found,
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by the test of nitrous air, that it was very little worse than common air; two measures of this air, and one of nitrous air, occupying the space of two measures only.

This fact will be thought a pretty remarkable one; and I can give no satisfactory account of it, unless the following should be deemed to be so. Fixed air, phlogisticated with iron-filings and brimstone, or with the electric spark, I have discovered, vol. I. p. 42. to become, in a much greater proportion than usual, immiscible with water; and I therefore concluded that this acid air (for such fixed air evidently is) by combining with phlogiston, comes to be a kind of air similar to common air. If this be a just account of the former experiment, the fixed air in this case must get phlogiston from the water with which it was combined, and thereby become, in part, immiscible with water. That water, even the purest, does contain phlogiston, is, I think, evident from the experiments which shew that air is injured by much agitation in it. Or, if an earthy matter, and not phlogiston, be necessary to the constitution of respirable air, as, I think, appears from my experiments on dephlogisticated air, there may be enough of it held in solution in the purest water, and which it may impart to the fixed air combined with it.

All water, which has been any time exposed to the atmosphere, contains more or less air, part of which is, I believe, always fixed air. This abounds so much in some mineral waters, that their peculiar virtues are certainly owing to this ingredient in their composition. This consideration has led some persons to ascribe the virtues of other mineral waters to this principle, though they contain it in so very small a proportion, as to make that opinion very improbable. Some, for instance, have thought that the virtues of the *Bath water* were owing, in a great measure, to the fixed air it contains; and living at no great distance from that celebrated spring, I thought I should incur a just censure, if I did not endeavour to ascertain what kind of air is contained in that water, and in what proportion. Accordingly, I made an excursion as far as Bath, chiefly with that view, and made the following experiments, which, having no apparatus of my own along with me, I was enabled to perform by the friendly zeal and ingenuity of Mr. Painter; Dr. Guffhart, Dr. Falconer, and Dr. Watson favouring me with their presence.

In order to ascertain what proportion of air is contained in the water, in the state in which it is drank, I filled a pint-phial with water, hot from the pump, and expelled the air from
it.

it, by boiling it about four hours, receiving the produce in quicksilver. This air was about $\frac{1}{30}$ of the bulk of the water, and about one-half of it was fixed air, precipitating lime in lime-water, and being readily absorbed by water. The residuum appeared, by the test of nitrous air, to be rather better than air in which a candle had burned out.

The quantity of fixed air that appears, by this experiment, to be contained in the Bath-waters is so very small, that I think it very improbable that their virtues should be at all owing to it. Few spring-waters, I believe, contain much less fixed air, and many I know, which have no medicinal virtue at all, contain more. The pump-water belonging to the house in which I now live, contains about $\frac{1}{14}$ of its bulk of fixed air; and it may be seen in my former volume, p. 160, that my pump-water at Leeds contained about $\frac{1}{30}$ of its bulk of air, of the very same composition as the air of the Bath-waters, viz. half of it fixed air, and half common air a little phlogisticated, so as to be in about the same state as air in which a candle had burned out.

Besides, the length of time which the Bath-waters, and indeed most other spring-waters, require to expel the air by means of heat, shews that

that the air expelled from them, was not contained in them in that state in which it is contained in waters properly impregnated with fixed air, out of which it may always be expelled by the heat of boiling water in less than an hour. In fact, the fixed air is not united to the *water*, but to some *calcareous matter* in the water, out of which the air is expelled with much more difficulty. Accordingly, Dr. Falconer informs me, that there is a deposit made by this water, after long boiling; if so, it may be presumed, that these waters do not so properly contain fixed air, as a calcareous earth; which, though it contain fixed air, may not part with it in the stomach, unless it meet with some acid to decompose it.

Besides the air contained in the Bath-water, there is a considerable quantity of air continually bubbling up from almost every part of the soil, through the water in the bath. When I was about to examine this air, Dr. Falconer informed me, that it had been done already by Dr. Nooth, and that an account of his experiments was inserted in the second volume of his treatise on the Bath-waters. The paragraph relating to it is as follows:

“ At the place where the springs rise in the
 “ baths, numerous bubbles of air are observed
 “ to

“ to ascend along with them. A quantity
 “ of air of this kind was collected at the King’s
 “ Bath, by inverting a glass, and holding it
 “ over the bubbles as they rose, and then
 “ conveying it into an inverted bottle, which,
 “ when full, was carefully corked up, and
 “ carried away. The air thus obtained an-
 “ swered in every respect to fixible air, preci-
 “ pitating lime in lime-water, and having
 “ every other quality which that substance
 “ possesses.”

Being informed of this, I thought it unnecessary to repeat the experiment; but finding, upon inquiry, that Dr. Nooth had not examined what proportion the residuum of the fixed air bore to the whole, or of what quality that residuum was, though he speaks of the whole as containing *every quality that fixed air possesses*, I thought it would not be amiss, as I was upon the spot, to make the trial myself. Accordingly, I took about a pint of that air, in nearly the same manner that Dr. Nooth had done, and found, upon examination, that only about $\frac{1}{20}$ of its bulk was fixed air, precipitating lime in lime-water, and being readily absorbed by water. The rest extinguished a candle, and was so far phlogisticated, that two measures of it, and one of nitrous air, occupied

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cupied the space of $2 \frac{1}{2}$ of a measure ; that is, it was almost perfectly noxious.

Had I had more leisure, and a better apparatus, the experiments might have been made with more accuracy ; but I do not think that, whenever they are repeated, they will be found to be materially wide of the truth, though it is possible that the state of the air in the water, and especially that which rises through the water, may be subject to variation. The measures were only estimated by the eye ; but then all who were present agreed very nearly in the same estimation.

Being in Germany, in the summer of the year 1774, we happened to pass by the famous spring of Seltzer-water, near Schwallbach, and also another very hot spring near the road from that place to Mentz. Through both these springs there was a bubbling of air, exactly similar to that in the Bath-waters ; but I had not time, or convenience, for making the same experiments upon them, and therefore contented myself with finding that the air of both of them extinguished a candle.

It is well known that all fermented liquors, that are not quite flat or vapid, contain fixed air ; and I had the curiosity to try, what proportion

portion of this air is contained in different kinds of wine, and in wines in different states. For this purpose, I took one of the phials with a ground-stopple and tube, represented fig. *e*, containing $1\frac{1}{2}$ of an ounce-measure, and filling it accurately with each species of wine, I plunged it into a vessel of water, which was set on the fire to boil, receiving the air in quicksilver. The air that I got from all kinds of fermented liquors was pure fixed air; but, except champaigne and cyder, it was in much less quantity than I expected; the results being as follows.

The quantity of air contained in

Madeira, was	-	$\frac{1}{100}$	of an ounce-measure.
Port of 6 years old	-	$\frac{1}{48}$	_____
Hock of 5 years	-	$\frac{1}{24}$	_____
Barrelled Claret	-	$\frac{1}{12}$	_____
Tokay of 16 years	-	$\frac{1}{20}$	_____
Champaigne of 2 years	2		_____
Bottled Cyder of 12 years	$3\frac{1}{4}$		_____

Some champaigne sparkles much in consequence of containing much air; but there is a kind of champaigne which does not sparkle, and contains very little air. The difference, as I was informed, when I made inquiry concerning it, in that part of France where the wine is made, is owing to this; that when they wish

to have the wine sparkle, they check the fermentation as much as possible at the time that the wine is made; so that the fermentation going on gradually, the fixed air produced by it is absorbed by the liquor: whereas, when they do not chuse to have it sparkle, they let it ferment freely, like any other kind of wine.

In other cases, therefore, where fermented liquors contain much air, as in most kinds of malt-liquor, cyder, and our English made-wines, I take it for granted, that the fermentation is either purposely checked, or that the liquor is of such a nature, that the fermentation will necessarily continue a long time, after it is put into the cask or bottle.

I once found that a quantity of port-wine contained its own bulk of fixed air; but I now imagine that the wine was not genuine, but must have been made chiefly of cyder. Perhaps this may not be a bad method of distinguishing genuine foreign wines from compositions made of cyder.

SECTION XIII.

Miscellaneous Observations.

I.

I have mentioned a fact, which shews that *chalk* retains fixed air very obstinately; so that neither the solar rays, nor the strongest heat of a smith's fire, continued for a long time, can expel the whole quantity that it contains. I have also found, that a small quantity of fixed air was contained in the best *quicklime* that I could procure; since strongly concentrated acids would still expel a small quantity from it. I mention this, chiefly, for the sake of an observation which may not be new, but which, if it be new, may be of some use, viz. that when I had heated some pieces of quicklime in oil of vitriol, in order to extract from it all the air that I possibly could, the next day I found the oil of vitriol solid and transparent, exactly resembling a thick jelly; but it became fluid again with the heat of my hand. This may, probably, be a good and expeditious method of concentrating this acid, the quicklime absorbing its water.

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II.

II.

I made a beginning of a course of experiments, which, I think, may be pursued to considerable advantage, on the state of the air which is contained in the *bladders of fishes*. It is commonly supposed, that these bladders are of no other use to the fishes than to assist them in rising or sinking in the water : but I have some doubt about this hypothesis ; at least they may have some other use. Some fishes, I believe, are not furnished with these bladders. When they are taken out of the fish, the air cannot be got from them by pressure, but I was always obliged to burst or cut them ; and yet that the air does change in these bladders, is, I think, pretty evident, from my having found it in different states.

The first time that it occurred to me to examine the air contained in these bladders, I found it, in a great number of them, to be perfectly noxious, not being at all affected by nitrous air. This was on the 31st of May, 1774. But at another time, viz. the 30th of March following, I found air that I had pressed from the bladders of the same kind of fishes, viz. roaches, not to be quite noxious, being affected by nitrous air, though not to a great degree.

degree. I have not pursued these experiments any farther; but I should think that it might not be difficult, by diversifying them properly, to make some discoveries concerning the animal œconomy of fishes, and the use of air to them.

III.

That excellent anatomist, Mr. John Hunter, told me, that fishes would not live in water impregnated with fixed air. I repeated the experiment, and found that small fishes would not live in this kind of water more than a few minutes. At the same time I had the curiosity to try how they would be affected by water impregnated with *nitrous air*, and observed that they were affected in the same manner, but much more violently; being thrown into the greatest agitation the moment they were put into it, and moving about with the greatest rapidity, till they became languid and died. A course of experiments of this kind, joined with the other, would, I think, be very promising to a person who had an opportunity of making them to advantage.

IV.

In some chymical processes, volatile alkali dissolves copper. This I also have observed

in my account of the experiments in which I put some pieces of volatile alkaline salt to a quantity of common air, at the time that I introduced nitrous air to it, vol. I. p. 213. For, if the alkaline salt be supported by copper-wire, it presently becomes blue, and is soon corroded. I therefore thought that pieces of copper, exposed to pure *alkaline air*, would have been affected in the same manner; but I did not find this to be the case. A number of pieces of copper-wire remained a whole night in alkaline air without sensibly affecting it, or being affected by it. That the alkaline air was pure, appeared by its being wholly absorbed by water afterwards.

V.

I had some expectation that alkaline air might be expelled from caustic *fixed alkali*, especially as it is known that the fixed and volatile alkalis differ only in their combinations; but I was disappointed in my expectations. Having procured a quantity of caustic alkali from Mr. Lane, who is known to prepare it with particular accuracy, I treated it in the same manner as I had done the spirit of salt, and found that the vapour expelled from it consisted of nothing but water, being immediately condensed when it came to the cold quicksilver.

VI. I

VI.

I have observed before, that though the marine acid air does not become inflammable air by means of liver of sulphur, as it does by means of many other substances that contain phlogiston; yet that it does form a permanent kind of air, which appeared to be phlogistified, by extinguishing a candle, though the quantity which I then produced was so small, that I did not pretend to form an accurate judgment concerning it. I have, since that, made another experiment of the same kind, rather more decisive than the former. I put several pieces of liver of sulphur to a quantity of marine acid air; when I observed that it presently began to be absorbed, and it continued in that state till one-half of the whole had disappeared. By this time the liver of sulphur, which had been of a greenish or yellowish colour, became white. Afterwards more liver of sulphur absorbed more of this air; but after two days the pieces began to dissolve, and at length they became one liquid mass, the air still diminishing very gradually. In this state I admitted water to the air; but by this very little more of it was absorbed; and that which remained was about one-fourth of the original quantity, and extinguished a candle. The whole process was three days. After this air had stood
a week

a week in water, and had been a little agitated in it, it was a little diminished by nitrous air.

VI.

I have mentioned a course of curious experiments on the mixing of *ether* with several kinds of air, the consequence of which was, that the quantity of each of them was almost instantly doubled by a single drop of that fluid; but that afterwards, water absorbed the ether, leaving the air possessed of all its peculiar properties. Those experiments were made with vitriolic ether. Having, since, procured a quantity of *nitrous ether*, made by Mr. Godfrey, I had the curiosity to try whether this would produce the same effect; but I found that it increased common air only about one-sixth of its bulk. After this mixture had continued two days and a night, water absorbed the ether, and left the common air exactly, or very nearly, the same as before, judging by the test of nitrous air.

VII.

In my experiments to extract air from fresh-made red lead, by mixing it with spirit of nitre, I had the curiosity to try what would be the effect of mixing it in the same manner with a volatile alkaline water; but no air was produced

duced from it, neither did the red lead acquire any additional weight from the mixture.

VIII.

Considering the very different properties of the different kinds of air with which I have been conversant, it was impossible not to think of the probability of their having different *refractive powers*, and of some method of ascertaining this circumstance. Accordingly I intended to have done something of this kind before the publication of my former volume on this subject; but I was prevented by an unexpected delay in the construction of the apparatus which I had contrived for that purpose. I have since completed my apparatus, and have made the trials which I then proposed; but I am sorry to inform my readers, that they have been without any success.

For this purpose, I procured a prism, consisting of three plates of glass, fastened together by cement, the cavity being large enough to contain about a quarter of a pint. This prism I fixed upon a stand, at the distance of ten feet from a window, in which I had a small apparatus, contrived to throw a beam of the sun's rays into the room. This beam was received by a board, furnished with a piece of brass-work, containing several small holes,
I through

through any of which I could transmit a beam of light upon the prism, which was placed, in a vertical position, close behind it; and the wall on which the image of the sun was received was twenty feet from the prism.

With this apparatus, which I thought promising enough, I proceeded to try the refractive powers of nitrous and inflammable air; but I could perceive no difference in the place of the image, whether the beam of light was transmitted through the prism, carefully filled with either of these kinds of air, or not through it; allowance being made for a small degree of refraction occasioned by a want of perfect parallelism in the plates of the prism. The result was the very same, whether it contained common air, or either of the two kinds above mentioned.

Having had so little success with these two very different kinds of air, I thought it would be in vain to try any of the other kinds; and therefore, for the present, have desisted from my pursuit; but I am not without a design to resume it with a different kind of apparatus, if I be so happy as to succeed in the construction of it.

IX.

The facility with which the nitrous acid forms air of various kinds is very remarkable; especially when compared with the two other mineral acids, which enter into the composition of few kinds of air in comparison with this. I was in hopes that, by substituting those acids in the place of the nitrous, in the experiments which produced the dephlogisticated, and other kinds of air, I should, at least, have got *some* kind of air; but I got none. I have mentioned my having tried this with red lead. I also made the same attempt with the marine acid, and dried flesh, from which I got the peculiar kind of air described sect viii.; but this produced nothing but the marine acid air, in quicksilver, and nothing at all in water; the acid air being absorbed by it as fast as it was generated. Trying a piece of *beef* with the same apparatus, without any acid, it yielded, by means of a pretty strong heat, from the flame of a candle, inflammable air, as in the similar experiment with a gun-barrel, mentioned in my former publication.

X.

I have observed, in the first volume of this work, that when I had put a piece of *salt-petre* to a quantity of marine acid air, it was pre-

sently dissolved, emitting a white fume; but that no air, that I could examine, remained, the quantity of it being so small. I have since repeated the experiment; but the result was nothing more than might have been predicted; for the nitrous acid, dislodged from its base by the marine, had dissolved some of the quick-silver, and formed nitrous air, occupying one-half of the whole space that had been filled by the marine acid air.

Marine acid air affects *borax*, in the very same manner in which alkaline air affects alum, rendering it whitish.

XI.

At the time of my former publication, I had found that taking the *electric spark* in given quantities of several kinds of air, had a very remarkable effect upon them, that it diminished common air, and made it noxious, making it deposit its fixed air, exactly like any phlogistic process; from whence I concluded that the electric matter either is, or contains phlogiston. It has also the same effect as a phlogistic process on nitrous air, diminishing it very much, and depriving it of its property of diminishing common air. I have since repeated this experiment on some other kinds of air which cannot be confined by water, and I find

find the results to be no less remarkable, though I have not given so much attention to them as to be able to explain them. The facts were as follows.

Having made about fifty electric explosions of a common jar, in a small quantity of the *marine acid air*, confined in a glass-syphon by quicksilver, I observed that it was a little diminished, and that a small part of the inside of the glass, next to the quicksilver, was tinged white. Water admitted to this air absorbed so much of it, that no experiment could be made on the remainder.

I made the same experiment on the *vitriolic acid air*; when presently the inside of the glass through which the explosion passed was uniformly covered with a blackish matter, so that nothing could be seen through it, and the air seemed to be rather increased than diminished. Water being admitted to it, left so little of it unabsorbed, that it could be no more examined than that which remained in the preceding experiment. Part of the blackish matter was washed off by the water.

I took the electric explosion in a small quantity of *alkaline air*, in the same manner as in the two preceding experiments, and observed, that

that every stroke added considerably to the quantity of air; and when water was admitted to it, just so much remained unabsorbed as had been added by the explosions. I then took about an hundred explosions of the same jar, in a larger quantity of alkaline air; after which, so much of it remained unabsorbed by water, that I could examine it with the greatest certainty. It neither affected common air, nor was affected by nitrous air, and was as strongly inflammable as any air that I had ever procured.

These experiments appear to me to furnish matter for much speculation, and farther experimental inquiry. Till this be done, all *conjecture* concerning them must be very much at random. I therefore defer making any at present.

SECTION XIV.

Experiments and Observations on CHARCOAL,
first published in the Philosophical Transac-
tions, vol. LX. p. 211.

Among the original experiments, published in the History of Electricity, was an account of the conducting power of charcoal. This substance had been considered by electricians, in no other light than that of more perfectly baked wood, which is known to be no conductor of electricity. I have even heard of attempts being made to excite it; and though those attempts were ineffectual, the failure of success was attributed to other causes than that of charcoal being no electric substance; so fixed was the persuasion, that water and metals were the only conducting substances in nature. The consideration of the chymical properties of charcoal, which are, in many respects, remarkably different from those of the wood from which it is made, might have led philosophers to suspect, that since, after its being reduced to a coal, it was become quite *another thing* from what it was before, it might possibly differ

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from

from wood in this property; but this consideration had not been sufficiently attended to.

In the account of my former experiments on charcoal, I observed, that there were very great differences in the conducting power of charcoal, and particularly of wood-charcoal, though I could not determine on what circumstances in the preparation, &c. those differences depended. I therefore expressed a wish, that some person, who had conveniencies for making chymical experiments, would prosecute the inquiry, as one that promised, not only to ascertain the cause of the conducting power of charcoal, but perhaps of *conducting power universally*. Not hearing that any chymist or electrician has attended to this business, I have, at length, resumed the subject, though not with every advantage that I could have wished. I have, in a great measure, however, succeeded in the principal object of my inquiry; and I shall now lay before this society the result of my experiments and observations.

I shall begin with correcting a mistake I lay under at the time that I made the former experiments. Having been informed by persons, who attend the making of *pit-charcoal*, that it was considerably increased in bulk after the process; I imagined that all other substances

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ces received an increase of bulk, when they were reduced to a coal; but the first experiments that I made, convinced me of my mistake. All vegetable substances are considerably contracted in all their dimensions, by the process of coaling, and the more perfect this process is (that is, as will be explained hereafter, the greater is the heat that is applied in the course of it) the greater is the diminution. I have even reduced pieces of wood to little more than one-fourth of their original length and breadth, in a common fire, by the use of a pair of hand-bellows only. And this was the case equally with wood of the firmest texture, as ebony; that of a middle texture, as oak; and that of the loosest, as fir, &c.

As moisture (and, I believe, small degrees of heat or cold) affects wood much more sensibly *across* the fibres than *along* them, it might have been supposed, that when wood was reduced to a coal, by the application of a greater degree of heat, the same rule would have been observed; but I found very little difference in this respect. To ascertain this circumstance, I took from the same board, two pieces, each $2\frac{1}{2}$ inches in length. In one of them, the fibres were divided, in the other they were not; and after coaling them thoroughly together, in the same crucible, I found that the former

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measured

measured 2.05 inches, and the latter 2.15. Their conducting power could not be distinguished.

A more particular account of the degree, in which wood is shortened in coaling, will be seen afterwards, when the variations in this respect are compared with the variations in the power of conducting electricity.

To my great surprize, I found *animal substances* not reduced in their dimensions by the process of coaling. This, at least, was the case with some pieces of *ivory*, several inches in length, and a piece of *bone*. They bore a very intense heat for many hours, and came out of the crucible considerably diminished in weight, but hardly so much as distorted in their shape, as is remarkably the case with wood, and, I believe, all vegetable substances.

In examining mineral substances, I found that my information, mentioned above, was just. Coals are very much enlarged in their dimensions by charring; but the experiment must be made with great care, to judge of this circumstance; for, unless the operation be very slow, the coal will retain nothing of its former shape, having been made, in some measure, fluid by the heat. The inside of all pieces of
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pit-charcoal is full of cavities, and there is generally a very large one in the center of every piece; so that the dilatation is nothing like the extension of fibres; but is produced by the elasticity of the new-formed vapour, in forcing its way out, while the substance is soft.

With respect to the main object of my inquiry, I presently satisfied myself, that the conducting power of charcoal depends upon no other circumstance than the *degree of heat*, that is applied in the process of making it. I had not suspected this; but numberless experiments clearly proved it. Taking an iron-pot, filled with sand, and putting into it pieces of wood, cut out of the same plank, marking them, and carefully noting their places in the pot, I always found that those pieces came out the best conductors, that had been exposed to the greatest heat. The result was the same when I made coals of bits of wood, placed one above another, in a gun-barrel, one end of which was made red-hot, and the rest gradually cooler and cooler.

Taking pieces of charcoal that conducted very imperfectly, or not at all, I never failed to give them the strongest conducting power, by repeating the process of coaling, either in

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a crucible,

a crucible, or in a gun-barrel, covered with sand, and kept in an intense heat.

I could not find that the mere continuance of the same degree of heat had any effect with respect to the conducting power of charcoal.

Mr. Macquer, and other chymists, define charcoal to be *wood burned, without being suffered to flame*; but, with respect to its conducting power, and, I make no doubt, with respect to all its other essential properties also, it makes no difference whether it flame or not. I have coaled pieces of wood, both in gun-barrels, and in crucibles, slightly covered with sand, and have let the inflammable vapour that exhaled from them take fire, at various distances from the substances; and I have also put pieces of wood in an open fire, and urged the heat applied to them, with a pair of bellows; and in all these cases have found the charcoal equally good. In the last method, indeed, very little of the substance is preserved; but the little that doth remain, after it hath ceased to flame, whether it be quenched immediately, or not, conducts as well as any charcoal whatever. But one can hardly be sure that the same degree of heat is given to every part of a piece of wood, except it be exposed to it for some time; and in an open fire, urged with a pair
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of bellows, the wood wastes as fast as it is red-hot, before the center of it is much affected with the heat.

When once any degree of conducting power is given to a piece of charcoal, I never found that it was afterwards lessened. A partial consuming of it in an open fire doth not affect the remainder, as I observed in the account of my former experiments.

I had imagined, that the *solidity* of substances converted into charcoal, would have had a very considerable effect on their conducting power afterwards; but the conjecture was not confirmed by experiment. Coals made of the lightest woods conducted, as far as I could perceive, as well as those that were made from the most solid, if they had been exposed to the same degree of heat in the process. Fine shavings of fir, the fine coats of an onion, the lightest foot, and every other vegetable substance that I tried, conducted equally with coals made of oak or ebony.

I had imagined, also, that the moment a piece of wood was become black with heat, it was, to all intents and purposes, a real charcoal; and, along with the other properties of charcoal, would conduct electricity, more or
R. 4 less;

less; but I found, by coaling several pieces very slowly, that they would not conduct in the least degree, not only when they were made superficially black, but likewise when they were black quite through, and had remained a long time in the heat that made them so; so that no eye could distinguish them from the most perfect charcoal.

I have sometimes found charcoal in such a state, that it would assist the passage of an explosion along its surface, when it would not conduct a shock any other way.

In order to satisfy myself in what proportions the *diminution of weight*, the *decrease of bulk*, and the *conducting power* of wood and charcoal, corresponded to one another, I took several pieces from the same plank, and having carefully weighed and measured them, converted them into coals very slowly, and by a gradual increase of heat, on an iron plate, held on the fire, turning them constantly, to prevent their catching fire. The following were the results.

A piece of very old dry oak, weighing 12 grains, and which conducted in the imperfect manner that wood generally does, from the moisture it contains, was, after the loss of about
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one grain; no conductor at all; and it continued the same as baked wood, till it was reduced to four grains, when it was black quite through; and even then, no part of it conducted, except one corner, where it had caught fire.

Another piece I carefully weighed, and measured several times in the course of the process. At first it weighed

Gr.		Length.	Bread.	Thick.
12	when its dimensions in inches were	2.	.45	.12
At 8	—————	2.	.4	.12
— 5 .5	—————	1.91	.4	.12
— 3 .5	—————	1.8	.35	

It was now become an imperfect conductor. I then urged it with a strong heat, in a crucible, and taking it out, it weighed 1 .75 gr. and measured 1 .6 in length, and .3 in the other dimensions. It was now a perfect conductor; and though I afterwards kept it in a very intense heat several hours, by which it was reduced to one grain in weight, its conducting power was not sensibly increased; but it was become very brittle, or friable.

It appears from these experiments, that these pieces of wood were reduced to about one-fourth of their weight before they would conduct

duct at all ; though, at the same time, they were diminished in length (*i. e.* along the fibres) only one-tenth. The breadth and thickness could not be measured with sufficient accuracy in these small pieces. To make them perfect conductors, they were reduced to about one-tenth in weight, and one-half in length.

A variety of circumstances led me to conclude, that the cause of *blackness*, and of the conducting power in charcoal, is the oil of the plant, made empyreumatic, and burnt to a certain degree. I therefore conclude that these properties are some way connected with that part of the inflammable principle, otherwise called phlogiston, that is fixed and united to the earth of the plant, when the union is strengthened by an intense heat.

The *sand*, with which I covered the substances that I converted into coals, and also the *pipe-clay* which I sometimes put over them, contracted a blackness like charcoal, and would often conduct pretty well. Sometimes they would conduct a shock. This must have been owing to the oil they received from the substances out of which it was expelled by the heat. In the experiment of the gun-barrel filled with pieces of wood, mentioned above, the uppermost pieces were not in the least burned.

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They could hardly have been hot ; yet, having contracted a superficial blackness, from the vapour of the oil expelled from the piece below them, they would even conduct a shock, though not in the most perfect manner.

Sometimes those substances that had no phlogiston themselves, but received it in consequence of being placed in the neighbourhood of other bodies out of which it was expelled, would not conduct immediately ; but would be made to do so by being exposed to a greater heat, which more thoroughly burned the oil with which their pores were filled.

I put a piece of a common *pipe* into a crucible, in which I was burning some turpentine (which will be mentioned below) and it came out black quite through, like a pipe in which tobacco has been frequently smoked. In this state it would not conduct at all ; but putting it into a crucible, covered with sand, I treated it in the same manner as I would have done a piece of wood, in order to coal it, and it came out a very good conductor. Had it been burned in the open fire, the phlogiston would have escaped, and the pipe would have been left white, as at first.

Being

Being convinced that the conducting power of charcoal depended upon the oil, or rather the phlogiston contained in the oil, and on the degree of heat with which it was burned, I took several methods to give vegetable substances more of this principle; or at least endeavoured to make them retain more of it than they usually do, in the process of coaling. But I had no apparent success in those experiments.

I began with plunging a piece of old dry oak in oil; and then, pumping the air out of it, let it stand *in vacuo* a day and night, in which time it seemed to discharge a great quantity of air; after which I let the air into the receiver, and thereby forced the oil into its pores. But the coal from this wood was not sensibly better than others. The application of heat may, perhaps, expel the phlogiston in such a manner, that the residuum, being fully saturated, can retain no more than a certain proportion. I made coals of other pieces of wood, when they were covered with cement; and I also coaled several pieces together, that they might receive phlogiston from one another; but, in both cases, without any sensible improvement in the quality of the coal.

In order to prevent the escape of the phlogiston belonging to the substance to be reduced
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to a coal, I put some pieces of wood into a gun-barrel, and corked it as close as I could, at the same time covering the cork with cement. In this case the rarefaction of the exhaling vapour never failed to drive the cork out; but it must have been after a considerable resistance to its escape. However, I could not perceive any peculiar excellence in the charcoal made in this manner.

I do not, indeed, know any method in which differences in substances that conduct so well as these can be accurately tried, at least none that can be applied in this case. The charcoal I can make in a common fire, by the use of a pair of hand-bellows, I cannot distinguish, with respect to its conducting power, from the most perfect metals, gold and silver; either by the length of the electric spark, the colour of it, or the sound of the explosion. I make no doubt but that wood, in the process of coaling, may easily have a degree of conducting power communicated to it, exceeding that of lead, iron, or the other more imperfect metals.

We may, perhaps, be guided in our conjectures on this subject, by considering the *degree of heat* that is necessary, either to unite the phlogiston to its base, or to separate them, both in the case of wood, and the different metals.

Lead

Lead is very easily calcined, and it is also known to conduct electricity very imperfectly. Iron soon turns to rust; and its conducting power I found to be very small, in comparison with that of copper, or the more perfect metals. If, therefore, in making charcoal, a degree of heat be applied greater than is necessary to calcine or revivify a metal, we may perhaps conclude, that the conducting power of the charcoal will be superior to that of the metal. As it may be possible to give charcoal, when cut off from any communication with the external air, a greater degree of heat than silver or gold would bear without being dissipated in vapour; it may even be possible to make charcoal that shall conduct electricity better than those most perfect metals.

Had there been any phlogiston in water, I should have concluded, that there had been no conducting power in nature, but in consequence of some union of this principle with some base. In this, metals and charcoal exactly agree. While they have the phlogiston, they conduct; when deprived of it, they will not conduct *.

* Having since found, that long agitation in the purest water injures air, so that a candle will not burn in it afterwards, which is precisely the effect of all *phlogistic processes*, I now conclude that the maxim suggested in this paragraph is universally true. See vol. I. p. 283.

I believe

I believe, however, that all vegetable or animal substances, that contain phlogiston, may be reduced to a coal; and if the heat applied in the process be sufficient, that coal will conduct electricity. Flesh, glue, bones, and other parts of an animal body, make good conducting charcoal.

The only approach, or seeming approach, I ever made towards retaining more phlogiston than usual, in wood reduced to a coal, was by the *slowness of the process*. For I always found, that if the heat was applied very gradually, less volatile phlogiston, *i. e.* less inflammable air, was expelled; and therefore I suppose that more of it was fixed. I could never afterwards, by equal degrees of heat, make this coal to weigh as little as another that was first coaled by a sudden heat.

I took two pieces of dry oak, the contiguous parts of the same stick, each weighing exactly fourteen grains. One of these I heated suddenly. It yielded eight ounce-measures of inflammable air, and then weighed two grains. The other I heated slowly, but as vehemently, at the last, as the other. It yielded only $1\frac{1}{2}$ ounce-measures, and weighed three grains.

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I repeated

I repeated the same experiment several times, and always with nearly the same result.

Examining the conducting power of the pieces of charcoal made with these different circumstances in the process, I could not distinguish which was better. Perhaps a more accurate method of trying them might show, that those which were coaled slowly were the better conductors; unless, which is not improbable, the goodness of the conducting power consists in the *completeness of the union* that is produced between the inflammable principle and its base, which will depend upon the *degree of heat* only, and not on the *quantity of phlogiston* thus united to the earth.

N. B. To catch the inflammable air, set loose in making charcoal, I put the substances into a gun-barrel, to which I luted a long glass tube, and to the tube I fastened a bladder, out of which the air was carefully pressed.

As metals and charcoal agree in consisting of phlogiston united to an earthy base, and also in conducting electricity, I suspected that these two different substances might also agree in their readiness to expand by heat. Mr. Smeaton was so obliging as to assist me in my attempts to ascertain this circumstance, by
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the application of his excellent pyrometer. Though we could not make the experiment with all the exactness that we could have wished, yet the result of near thirty trials was uniformly in favour of the greater degree of expansion, by heat, in the charcoal, than in wood of the same kind (as we imagined) out of which it was made. In general, the expansion of the charcoal was about double to that of the wood.

It is evident, that a certain degree of heat makes wood and charcoal expand; and also that a greater degree of heat makes them contract. I wish we had an instrument to ascertain the precise degree of heat, at which the expansion ceases, and the contraction begins; and whether the two effects be produced by the same gradation.

In the course of these experiments on charcoal, I met with a substance, the conducting power of which is singular, and exhibits a beautiful appearance. In order to see what would remain after burning a quantity of turpentine in a glass tube, I covered it with sand, in a crucible, in the same manner in which I used to make charcoal; and, after letting it continue a sufficient time, in a very hot fire, and after the flame had long ceased, I examined the tube, and found that it had been melted; but,

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instead of any thing like charcoal, or the least blackness, I observed that the tube was uniformly lined with a *whitish glossy matter*, which I could not scrape off. Upon trying whether it would conduct electricity, I found it transmitted the smallest shocks, to a considerable distance; and, what appeared very remarkable, the path of the explosion was luminous all the way, and seemed to consist of a prodigious number of small separate sparks, scattered to a great distance, exhibiting such an appearance as would be made by firing gunpowder scattered carelessly in a line. The explosion very much resembled the firing of a squib. To compare it to another electric appearance, it was like the explosion passing over a thin surface of gilding.

I imagine that, though I could not perceive any interruption in this white coating, not even by the help of a microscope, it must, in fact, have been full of interstices, and the electric sparks could only be visible in passing from one conducting particle to another.

In this experiment, I often got pieces of glass very imperfectly covered, with intervals in the white coating very large and visible; but, though I exposed the same pieces of glass to catch more of this matter, I never could get

a coating of it so thick, but that, in transmitting the electrical explosion through it, it exhibited the same luminous appearance, as if there were interstices in the circuit.

I got the same matter from oil of *turpentine*, and *oil of olives*; but not from *bees-wax*, or *spermaceti oil*. Perhaps it cannot be got from any animal substance.

In order to observe the progress of this incrustation, I poured oil of turpentine on some flat pieces of glass, and burned them on an iron plate, in the open fire, the heat being moderate; but the effect was a black covering, like soot, which would not conduct in the least. But these same pieces of glass, thus covered with the black coating, being put into a crucible full of sand, and urged with a strong heat, came out white, and conducted exactly as before.

With a less degree of heat, the black covering was changed to white; but it did not adhere so firmly to the glass as when the heat had been greater; though it adhered more closely than the black covering, which might be wiped off with a feather. But this white coating, produced by a moderate heat, would not conduct at all.

In some cases I have found this whitish matter to be dispersed by several explosions, as Dr. Franklin found gilding with leaf-gold to be.

In whatever manner the pieces of glass were covered, the coating vanished when it was made red-hot in an open fire; and the glass that remained would not conduct, any more than it did before. This circumstance exactly resembled the escape of phlogiston from charcoal and metal, burned in the open air.

In a microscope, this whitish matter looked exactly like metal, or rather some of the semi-metals, having a bright polish, though it soon became, as it were, tarnished.

To try whether it was metal, I dipped the pieces of glass that were covered with it in the *acids*, but found that they had little or no effect upon it, though it is by no means fixed in the pores of the glass, but covers it quite superficially.

It was not in the least affected by the *magnet*. Upon the whole, the matter that forms this coating of the glass seems to be a kind of charcoal, only white instead of black.

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Considering that metals resemble charcoal, in that they consist of an earth united to phlogiston, and that charcoal will not consume without burning in the open air (there being, probably, something in the atmosphere with which it can unite, on the principle of chymical affinities, the moment it is separated from the metallic base) I imagined that metals might not calcine, or vitrify, except in the same circumstances, and the event verified my conjecture.

I took a certain quantity of *lead*, and having put it into an open crucible, observed that it was all vitrified in ten minutes; but the same quantity of lead, covered with pipe-clay, and sand, was kept several hours in a much hotter fire, and was hardly wasted at all, the bottom of the crucible only being slightly glazed; it having been impossible wholly to exclude all access of air, and some being necessarily in contact with it when the process began. Treating charcoal in the same manner, I could never prevent some loss of weight, when the crucible was kept in a very hot fire, for several hours.

As, by this process, lead will bear a much greater degree of heat than would calcine, or vitrify it, in the open air, I should think it probable, that lead thus prepared must have

the phlogiston more closely united to its earthy base, and be thereby a better conductor than common lead; since this is the case with charcoal thus treated. Perhaps lead, and other base metals, may have their *quality* altered, and be improved in other respects also by this process; though they should not be changed into gold by it. I found, however, that the specific gravity is not changed by this process; so that, alas! it is still but lead.

Fig. 1

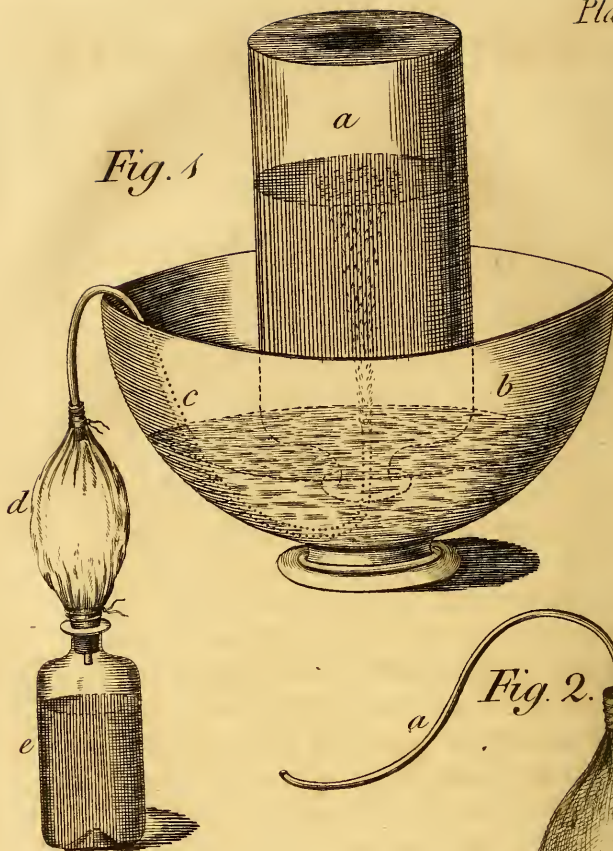


Fig. 2.

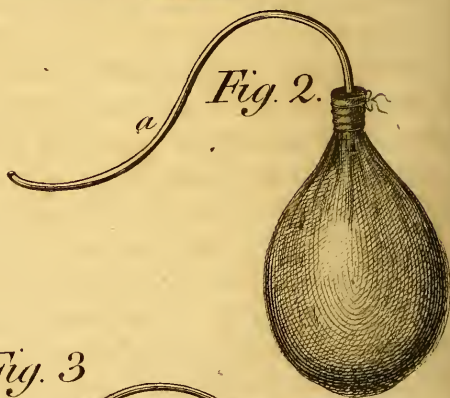


Fig. 3



SECTION XV.

Of the Impregnation of WATER with FIXED AIR.

PART I.

The History of the Discovery.

It often amuses me when I review the history of experimental philosophy, to observe how very nearly one discovery is connected with another, and yet that, for a long time, no person shall have perceived that connection, so as to have been actually led from the one to the other; and especially that he who made the first discovery should stop short in his progress, and not advance a single step farther, to make the other, which was perhaps of infinitely more consequence. And yet the case may be such, that it shall be so far from requiring more genius, or ingenuity, to advance that other step, that it is rather a matter of wonder, how it was possible for the most common capacity to stop short of it. We also frequently find that they who make the most important philosophical discoveries overlook the most obvious *uses* of them. Several striking examples of

this kind will be found in my *History of electricity*, and also in the *History of discoveries relating to vision, light, and colours*.

In such cases as these it behoves an historian to be much upon his guard, lest he should hastily conclude that to have been fact which he only *imagines* must have been so, but for which no direct evidence can be produced. As this is a case of some curiosity respecting the human mind, I shall give an instance of it; and I am able to produce a very remarkable one relating to the subject of this section.

When it was discovered that the acidulous taste and peculiar virtues of Pymont water, and other mineral waters of a similar nature, were owing to the fixed air which they contained; when this air had been actually expelled from the water, and it was found that the same water, and even other water, would reimbibe the same air; we are apt to conclude, that the person who made these discoveries, and especially the last of them (who also must have known that fixed air is a thing very easy to be procured) must have immediately gone to work to reduce this *theory* into *practice*, by actually impregnating common water with fixed air, in order to give it the peculiar virtues of those medicinal mineral waters which
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are so highly, and so justly valued, and which are procured at so great an expence, especially in this country. Accordingly, Dr. Nooth has advanced, Phil. Transf. vol. 65, p. 59, that “the possibility of impregnating water with fixed air was no sooner ascertained by experiment, than various methods were contrived to effect the impregnation;” and I doubt not this ingenious philosopher imposed upon himself in the manner described above. This, however, is so far from being the case, that I do not believe it is possible to produce the least evidence that any person had the thing in view before the publication of my pamphlet upon that subject, in the year 1772.

Indeed, had this thing been so much as *an object of attention* to philosophers, it is impossible but that some of them must have hit upon a method that would have sufficiently succeeded. Nay, the thing is so very easy, and the end attainable in so many ways, that there must have been, in a very short time, a great variety of methods to impregnate water with fixed air, as there are now; and we should certainly have heard of *artificial mineral waters* being made according to them. It is impossible not to conclude so, when we consider the *time that has elapsed* since the publication of all the discoveries that led to it.

Dr.

Dr. Brownrigg's paper, giving an account of his discovery of fixed air in the Spa water, was read at the Royal Society June the 13th 1765, and was published in 1766. This excellent philosopher compleatly decomposed that mineral water, but he gives no hint of his having so much as attempted to *recompose* it, or of making a similar water, by impregnating common water with the same volatile principle. It is sufficiently evident that he had not thought of this, though we may wonder that he should not have done it, because he has not mentioned it as an object of pursuit.

In the year following, Mr. Cavendish's valuable papers on the subject of factitious air were published. He first ascertained how much fixed air a given quantity of water could be made to imbibe; yet it does not appear that he ever thought of *tasting* the water, much less that he thought of making any *practical use* of his discovery.

If any negative argument can be decisive, it is that in 1772, the very year in which my pamphlet came out, Dr. Falconer published his excellent and elaborate treatise on the *Bath waters*, in which he treats very largely of mineral waters in general, and all their possible
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impregnations; and yet, though he treats of *fixed air* as one ingredient in many of them, see p. 185, he drops no hint about composing such water, by imparting fixed air to common water. Also on the 12th of September in the same year, Dr. Rutherford published his ingenious *Dissertation on Fixed Air*, in which he speaks of the presence of it in Pyrmont water, p. 3, but without giving the least hint of his being acquainted with any method of imitating them. And yet Dr. Nooth says, in fact, that from the year 1766, at the latest, *various methods* were contrived to effect the impregnation, though he allows that I was the only person who “published any description of an “apparatus calculated entirely for this purpose.”

According to this account of the matter there were, in the interval between 1766 and 1772, a space of six years, a variety of methods for impregnating water with fixed air, some of them prior to, and perhaps much better than mine (though he gives no hint of his own having been invented in that period, but speaks of it as suggested by the consideration of the imperfection of mine) but that I happened to get the start in the publication. Dr. Falconer, however, though the friend of Dr. Nooth (see his *Treatise on Bath Water*,
vol.

vol. 2. p. 323) had certainly never heard of any of those methods, or even of mine, at the very termination of that period; and tho' my own acquaintance with philosophical and medical people is pretty extensive, I never heard of any of the *various methods* that Dr. Nooth speaks of; nor since the publication of my method have I heard of any person whatever having pretended to have done the same thing before; though nothing is more common than such claims, and very often on the most trifling pretences.

Mr. Venelle, indeed, immediately upon the translation of my pamphlet into French, which was within a few weeks after the publication of it in English (owing to the laudable zeal of Mr. Trudaine, for promoting all philosophical and useful improvements) published an extract of his papers from the *Memoires de Mathematique & de Physique*, to vindicate to himself not my discovery, but, in fact, that of Dr. Brownrigg. However, what he pretends to have discovered was, that the virtues of the acidulous waters were owing to *air, in general*, without having any idea of the difference between fixed air and common air; so that his discovery was so far from being the same with mine, that it could not possibly have led into it.

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As I have hitherto only published the method of impregnating water with fixed air in a small pamphlet, for the use of those who might chuse to reduce it into practice, without giving any account of the manner in which the discovery (if it deserves to be called one) was made, which has been my custom with respect to every thing else, I shall do it here; and I hope the narrative will not be altogether displeasing, as this business has gained so much attention in all parts of Europe, as well as in England, and promises in a short time to save the very great expence of transporting acidulous waters to considerable distances, by superseding, in a great measure, the use of them. And though what I have done in this business has certainly the least merit possible with respect to *ingenuity*, I shall always consider it as one of the *happiest* thoughts that ever occurred to me; because it has proved to be of very signal *benefit* to mankind, and will, I doubt not, be of much more consequence in a course of time.

It was a little after Midsummer in 1767, that I removed from Warrington to Leeds; and living, for the first year, in a house that was contiguous to a large common brewery, so good an opportunity produced in me an inclination to make some experiments on the
fixed.

fixed air that was constantly produced in it. Had it not been for this circumstance, I should, probably, never have attended to the subject of air at all. Happening to have read Dr. Brownrigg's excellent paper on the Spa water about the same time, one of the first things that I did in this brewery was to place shallow vessels of water within the region of fixed air, on the surface of the fermenting vessels; and having left them all night, I generally found, the next morning, that the water had acquired a very sensible and pleasant impregnation; and it was with peculiar satisfaction that I first drank of this water, which I believe was the first of its kind that had ever been tasted by man.

This process, however, was very slow. But after some time it occurred to me, that the impregnation might be accelerated, by pouring the water from one vessel into another, while they were both held within the sphere of the fixed air; and accordingly I found that I could do as much in about five minutes in this way, as I had been able to do in many hours before. Several of my friends who visited me while I lived in that house will remember my taking them into that brewery, and giving them a glass of this artificial Pyrmont water, made in their presence. Among others, I will
take

take the liberty to mention John Lee, Esq; of Lincoln's Inn, who was particularly struck with the contrivance, and the effect of it. This was in the summer of the year 1768.

One would naturally think, that having actually impregnated common water with fixed air, produced in a brewery, I should immediately have set about doing the same thing with air set loose from chalk, &c. by some of the stronger acids; and I do remember that it did occur to me that the thing was possible. But, easy as the practice proved to be, no method of doing it at that time occurred to me. I still continued to make my Pymont water in the manner abovementioned till I left that situation, which was about the end of the summer 1768; and from that time, being engaged in other similar pursuits, with the result of which the public are acquainted, I made no more of the Pymont water till the spring of the year 1772.

In the mean time I had acquainted all my friends with what I had done, and frequently expressed my wishes that persons who had the care of large *distilleries* (where I was told that fermentation was much stronger than in common breweries) would contrive to have vessels of water suspended within the fixed air, which
they

they produced, with a farther contrivance for agitating the surface of the water; as I did not doubt but that, by this means, they might, with little or no expence, make great quantities of Pyrmont water; by which they might at the same time both serve the Public; and benefit themselves. For I never had the most distant thought of making any advantage of the scheme myself.

In all this time, viz. from 1767 to 1772, I never heard of any method of impregnating water with fixed air but that abovementioned. My thinking at all of reducing to practice any method of effecting this, by air dislodged from chalk, and other calcareous substances, was owing to a mere accident. Being at dinner with the Duke of Northumberland, in the spring of the year last mentioned, his Grace produced a bottle of water distilled by Dr. Irving for the use of the navy. This water was perfectly sweet, but, like all distilled water, wanted the briskness and spirit of fresh spring water; when it immediately occurred to me that I could easily mend that water for the use of the navy, and perhaps supply them with an easy and cheap method of preventing or curing the sea scurvy; viz. by impregnating it with fixed air. For having been busy about a year before with my experiments on

air, in the course of which I had ascertained the proportional quantity of several kinds of air that given quantities of water would take up, I was at no loss for the *method* of doing it in general, viz. inverting a jar filled with water, and conveying air into it from bladders previously filled with air. This scheme I immediately mentioned to the Duke and the company, who all seemed to be much pleased with it, and expressed their wishes that I would attend to it, and endeavour to reduce it into practice; which I promised to do.

The next day I provided a small apparatus, adapted to this purpose, at my lodgings, which was very easy, as it required no other vessels but such as are in constant family use, and with this I presently impregnated a quantity of the New River water, so as to make it imbibe about its bulk of air. But I was far from having hit upon the *easiest method* of doing it; for my jars were of an equal width throughout. However, with these vessels the process was completed in about twenty minutes, or half an hour.

A few days after this, having an invitation to wait upon Sir George Savile, I carried with me a bottle of my impregnated water, and told him the use that might be made of it,

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viz. that of supplying a pleasant and wholesome beverage for seamen, and such as might probably prevent or cure the sea-scurvy. Sir George, with that warmth with which he espouses every thing that he conceives to be for the public good, insisted upon writing a card immediately to Lord Sandwich, proposing to introduce me to him, as having *a proposal for the use of the navy*. As I could make no objection, the card was accordingly written, and an answer was presently returned from his Lordship, informing us that he would be glad to see us the next day. Upon this I drew up something in the form of a *proposal*, which, accompanied by Sir George, I presented to his Lordship, who promised to lay it before the Board of Admiralty.

Presently after this I had notice from the Secretary to the Board of Admiralty, that the *College of Physicians* were appointed to examine my proposal, and to make their report of it to the Board, and an early day was fixed for me to wait upon them at their hall in Warwick-Lane; where, before a very full meeting, I produced a bottle of my impregnated water, and also, at their request, fetched my apparatus, and shewed them the manner in which I had impregnated it. There were present several of the most eminent physicians in London;

London; but both the *scheme*, and the *object* of it, appeared to be intirely new to every one of them; and most of them seemed to be much pleased with it.

Accordingly, a favourable report was made to the Board of Admiralty, and I was acquainted by the Secretary, that the captains of the two ships which were just then sailing for the South-Seas had orders to make trial of the impregnated water; and for their use I drew out my *Directions* in writing, and sent a drawing of the necessary apparatus. The method which I had now got into was a great improvement upon that which I had made use of before the College of Physicians. For, in consequence of giving more attention to it, I had, by that time, brought it to the state in which it is described in the pamphlet.

In the mean time, I had, before I left London, in the spring of that year, made the experiment of the impregnation of water with fixed air in the presence of most of my philosophical acquaintance, and their friends, both at my own lodgings, and in other places. But upon none of these occasions did it appear that any of them had heard of any other person having had the same thing in view.

Lastly, I will observe, that Sir John Pringle, in his *Discourse on different kinds of air* (in which he has, with the greatest exactness, assigned to every person concerned in these discoveries their due share of praise) gives no hint of his being acquainted with any other method of impregnating water with fixed air, than that which I had published. He certainly had not heard of any of those to which Dr. Nooth alludes.

As I have not to this day, directly or indirectly, made the least advantage of this scheme; but, on the contrary, am just so much a loser by it as the experiments cost me, I think it is not too much for the Public to allow me, what I believe is strictly my due, *the sole merit of the discovery*; which with respect to *ingenuity*, or sagacity, is next to nothing; but with respect to its *utility* is, unquestionably, of unspeakable value to my country and to mankind.

P A R T

P A R T II.

DIRECTIONS for impregnating WATER
with FIXED AIR.

SECT. I. *The Preface to the Directions as first
published.*

The method of impregnating water with fixed air, of which a description is given in this pamphlet, I hit upon in a course of experiments, an account of which was lately communicated to the Royal Society; containing observations on several different kinds of air, with only a hint of the method of combining this particular kind with water or other fluids. Judging that water thus impregnated with fixed air must be particularly serviceable in long voyages, by preventing or curing the sea-scurvy, according to the theory of Dr. Macbride, and all the Physicians of my acquaintance concurring with me in that opinion, I made the first communication of it to the Lords of the Admiralty, who referred me to the College of Physicians; and those gentlemen being pleased to make a report favourable to the scheme, a trial has been ordered to

be made of it on board some of his majesty's ships. To make this process more generally known, and that more frequent trials may be made by water thus medicated, at land as well as at sea, I have been induced to make the present publication.

Sir John Pringle first observed, that putrefaction was checked by fermentation, and Dr. Macbride discovered that this effect was produced by the fixed air which is generated in that process, and upon that principle recommended the use of *wort*, as supplying a quantity of this fixed air, by fermentation in the stomach, in the same manner as it is done by fresh vegetables, for which he, therefore, thought that it would be a substitutē; and experience has confirmed his conjecture. Dr. Black found that lime-stone, and all calcareous substances, contain fixed air, that the presence of it makes them what is called *mild*, and that the deprivation of it renders them *caustic*; Dr. Brownrigg farther discovered that Pyrmont, and other mineral waters, which have the same acidulous taste, contain a considerable proportion of this very kind of air, and that upon this their peculiar spirit and virtues depend; and I think myself fortunate in having hit upon a very easy method of communicating this air to any kind of water, or, indeed,

deed, to almost any fluid substance. In short, by this method this great antiseptic principle may be administered in a variety of agreeable vehicles.

If this discovery (though it doth not deserve that name) be of any use to my countrymen, and to mankind at large, I shall have my reward. For this purpose I have made the communication as early as I conveniently could, since the latest improvements that I have made in the process; and I cannot help expressing my wishes, that all persons, who discover any thing that promises to be generally useful, would adopt the same method.

SECT. 2. *The Directions.*

If water be only in contact with fixed air, it will begin to imbibe it, but the mixture is greatly accelerated by agitation, which is continually bringing fresh particles of air and water into contact. All that is necessary, therefore, to make this process expeditious and effectual, is first to procure a sufficient quantity of this fixed air, and then to contrive a method by which the air and water may be strongly agitated in the same vessel, without any danger of admitting the common air to them; and this

is easily done by first filling any vessel with water, and introducing the fixed air to it, while it stands inverted in another vessel of water. That every part of the process may be as intelligible as possible, even to those who have no previous knowledge of the subject, I shall describe it very minutely, subjoining several remarks and observations relating to varieties in the process, and other things of a miscellaneous nature.

The Preparation.

Take a glass vessel, *a*, pl. 2. fig. 1. with a pretty narrow neck, but so formed, that it will stand upright with its mouth downwards, and having filled it with water, lay a slip of clean paper, or thin pasteboard, upon it. Then, if they be pressed close together, the vessel may be turned upside down, without danger of admitting common air into it; and when it is thus inverted, it must be placed in another vessel, in the form of a bowl or basin, *b*, with a little water in it, so much as to permit the slip of paper or pasteboard to be withdrawn, and the end of the pipe *c* to be introduced.

This pipe must be flexible, and air-tight, for which purpose it is, I believe, best made
of

of leather, sewed with a waxed thread, in the manner used by shoe-makers. Into each end of this pipe a piece of a quill should be thrust, to keep them open, while one of them is introduced into the vessel of water, and the other into the bladder *d*, the opposite end of which is tied round a cork, which must be perforated, the whole being kept open by a quill; and the cork must fit a phial *e*, two thirds of which should be filled with chalk just covered with water.

I have since, however, found it most convenient to use a *glass tube*, and to preserve the advantage which I had, of agitating the vessel *e*, I have *two bladders*, communicating by a perforated cork, to which they are both tied. For one bladder would hardly give room enough for that purpose.

The Process.

Things being thus prepared, and the phial containing the chalk and water being detached from the bladder, and the pipe also from the vessel of water, pour a little oil of vitriol upon the chalk and water; and having carefully pressed all the common air out of the bladder, put the cork into the bottle presently after the
effe-

effervescence has begun. Also press the bladder once more after a little of the newly generated air has got into it, in order the more effectually to clear it of all the remains of the common air; and then introduce the end of the pipe into the mouth of the vessel of water as in the drawing, and begin to agitate the chalk and water briskly. This will presently produce a considerable quantity of fixed air, which will distend the bladder; and this being pressed, the air will force its way through the pipe, and ascend into the vessel of water, the water at the same time descending, and coming into the bason.

When about one half of the water is forced out, let the operator lay his hand upon the uppermost part of the vessel, and shake it as briskly as he can, not to throw the water out of the bason; and in a few minutes the water will absorb the air; and taking its place, will nearly fill the vessel as at the first. Then shake the phial containing the chalk and water again, and force more air into the vessel, till, upon the whole, about an equal bulk of air has been thrown into it. Also shake the water as before, till no more of the air can be imbibed. As soon as this is perceived to be the case, the water is ready for use; and if it be not used immediately, should be put into a bottle as soon

soon as possible, well corked, and cemented. It will keep, however, very well, if the bottle be only well corked, and kept with the mouth downwards.

Observations.

1. The bason may be placed inverted upon the vessel full of water; with a slip of paper between them, and then both turned upside down together; but all this trouble will be saved by having a larger vessel of water, in which both of them may be immerfed.

2. If the vessel containing the water to be agitated be large, it may be most convenient first to place it inverted, in a bason full of water, and then to draw out the common air by means of a syphon, either making use of a syringe, or drawing it out with the mouth. In this case, also, some kind of handle should be fastened to the bottom of the vessel, for the more easy agitation of it.

3. A narrow mouthed vessel is not necessary, but it is the most proper for the purpose, because it may be agitated with less danger of the common air getting into it.

4. The

4. The flexible pipe is not necessary, though I think it is exceedingly convenient. When it is not used, a bent tube, *a*, fig. 2. (for which glass is the most proper) must be ready to be inserted into the hole made in the cork, when the bladder containing the fixed air is separated from the phial, in which it was generated. The extremity of this tube being put under the vessel of water, and the bladder being compressed, the air will be conveyed into it, as before.

5. If the use of a bladder be objected to, though nothing can be more inoffensive, the phial containing the chalk and water must not be agitated at all, or with the greatest caution; unless a small phial, *a*, fig. 3. be interposed between the phial and the vessel of water, in the manner represented in the drawing. For by this means the chalk and water that may be thrown up the tube *b* will lodge at the bottom of the phial *a*, while nothing but the air will get into the pipe *c*, and so enter the water. If the tube *b* be made of tin or copper, the small phial *a* will not need any other support, the cork into which the extremities of both the tubes are inserted being made to fit the phial very exactly.

6. The

6. The phial *e*, fig. 1. should always be placed, or held, considerably lower than the vessel *a*; that if any part of the mixture should be thrown up into the bladder, it may remain in the lower part of it, from which it may be easily pressed back again. This, however, is not necessary, since if it remain in the lower part of the bladder, nothing but the pure air will get into the pipe, and so into the water.

7. If much more than half of the vessel be filled with air, there will not be a body of water sufficient to agitate, and the process will take up much more time.

8. If the chalk be too finely powdered, it will yield the fixed air too fast.

9. After every process, the water to which the chalk is put must be changed.

10. It will be proper to fill the bladder with water once every day, after it has been used, that any of the oil of vitriol which may have got into it, and would be in danger of corroding it, may be thoroughly diluted.

11. The vessel, which I have generally made use of, holds about three pints, and the phial containing the chalk and water is one of ten ounces;

ounces ; and I find that a little more than a tea-spoonful of oil of vitriol is sufficient to produce as much air as will impregnate that quantity of water.

12. If the vessel containing the water be larger, the phial containing the chalk and the oil of vitriol should either be larger in proportion, or fresh water and oil of vitriol must be put to the chalk, to produce the requisite quantity of air.

13. In general, the whole process does not take up more than about a quarter of an hour, the agitation not five minutes ; and in nearly the same time might a vessel of water, containing two or three gallons, or indeed any quantity that a person could well shake, be impregnated with fixed air, if the phial containing the chalk and oil of vitriol, be larger in the same proportion.

14. To give the water as much air as it can receive in this way, the process may be repeated with the water thus impregnated. I generally chuse to do it two or three times, but very little will be gained by repeating it oftener ; since, after some time, as much fixed air will escape from that part of the surface of the

water which is exposed to the common air, as can be imbibed from within the vessel.

15. All calcareous substances contain fixed air, and any acids may be used in order to set it loose from them; but chalk and oil of vitriol are, both of them, the cheapest, and, upon the whole, the best for the purpose.

16. It may possibly be imagined that part of the oil of vitriol is rendered volatile in this process, and so becomes mixed with the water; but it does not appear, by the most rigid chymical examination, that the least perceivable quantity of the acid gets into the water in this way; and if so small a quantity as a single drop of oil of vitriol be mixed with a pint of water (and a much greater quantity would be far from making it less wholesome) it might be discovered. The experiments which were made to ascertain this fact were made with *distilled water*, the disagreeable taste of which is not taken off, in any degree, by the mixture of fixed air. Otherwise, distilled water, being clogged with no foreign principle, will imbibe fixed air faster, and retain a greater quantity of it than other water. In the experiments that were made for this purpose, I was assisted by Mr. Hey, a surgeon in Leeds, who is well skilled

skilled in the methods of examining the properties of mineral waters.

17. Doctor Brownrigg, who made his experiments on Pyrmont water at the spring head, never found that it contained so much as one half of an equal bulk of air; but in this method the water is easily made to imbibe an equal bulk. For it must be observed, that a considerable quantity of the most soluble part of the air is incorporated with the water, as it first ascends through it, before it occupies its place in the upper part of the vessel.

18. The heat of boiling water will expel all the fixed air, if a phial containing this impregnated water be held in it; but it will often require above half an hour to effect it completely.

19. If any person would chuse to make this medicated water more nearly to resemble genuine Pyrmont water, Sir John Pringle informs me, that from eight to ten drops of *Tinctura Martis cum spiritu salis* must be mixed with every pint of it. It is agreed, however, on all hands, that the peculiar virtues of Pyrmont, or any other mineral water which has the same brisk or acidulous taste, depend not
upon

upon its being a chalybeate, but upon the fixed air which it contains,

But water impregnated with fixed air does of itself dissolve iron, as the ingenious Mr. Lane has discovered; and iron filings put to this medicated water make a strong and agreeable chalybeate, similar to some other natural chalybeates, which hold the iron in solution by means of fixed air only, and not by means of any acid; and these chalybeates, I am informed, are generally the most agreeable to the stomach.

20. By this process may fixed air be given to wine, beer, and almost any liquor whatever: and when beer is become flat or dead, it will be revived by this means; but the delicate agreeable flavour, or acidulous taste communicated by the fixed air, and which is manifest in water, will hardly be perceived in wine, or other liquors which have much taste of their own.

21. I would not interfere with the province of the physician, but I cannot intirely satisfy myself without taking this opportunity to suggest such hints as have occurred to myself, or my friends, with respect to the *medicinal uses*

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of

of water impregnated with fixed air, and also of fixed air in other applications.

In general, the diseases in which water impregnated with fixed air will most probably be serviceable, are those of a *putrid* nature, of which kind is the *sea-scurvy*. It can hardly be doubted, also, but that this water must have all the medicinal virtues of Pyrmont water, and of other mineral waters similar to it, whatever they be; especially if a few iron filings be put to it, to render it a chalybeate, like genuine Pyrmont water. It is possible, however, that, in some cases, it may be desirable to have the *fixed air* of Pyrmont water, without the *iron* which it contains.

Having this opportunity, I shall also hint the application of fixed air in the form of *clysters*, which occurred to me while I was attending to this subject, as what promises to be useful to correct putrefaction in the intestinal canal, and other parts of the system to which it may, by this channel, be conveyed. It has been tried once by Mr. Hey above-mentioned, and the recovery of the patient from an alarming putrid fever, when the stools were become black, hot, and very fetid, was so circumstanced, that it is not improbable but that it might be owing, in some measure, to those clysters.

clysters. The application, however, appeared to be perfectly easy and safe.

I cannot help thinking that fixed air might be applied externally to good advantage in other cases of a putrid nature, even when the whole system was affected. There would be no difficulty in placing the body so, that the greatest part of its surface should be exposed to this kind of air; and if a piece of putrid flesh will become firm and sweet in that situation, as Dr. Macbride found, some advantage, I should think, might be expected from the same antiseptic application, assisted by the *vis vitæ*, operating internally, to counteract the same putrid tendency. Some Indians, I have been informed, bury their patients, labouring under putrid diseases, up to the chin in fresh mould, which is also known to take off the fætor from flesh meat beginning to putrify. If this practice be of any use, may it not be owing to the fixed air imbibed by the pores of the skin in that situation? Following the plough is also an old prescription for a consumption, as also is living near lime kilns. There is often some good reason for very old and long continued practices, though it is frequently a long time before it be discovered, and the *rationale* of them satisfactorily explained.

Being no physician, I run no risque by throwing out these random hints and conjectures. I shall think myself happy, if any of them should be the means of making those persons, whom they immediately concern, attend more particularly to the subject. My friend Dr. Percival has for some time past been employed in making experiments on fixed air, and he is particularly attentive to the medicinal uses of it; and from his knowledge as a philosopher, and skill in his profession, I have very considerable expectations.

P A R T. III.

Of Dr. NOOTH's Objections to the preceding Method of impregnating Water with fixed Air, and a Comparison of it with his own Method, both as published by himself, and as improved by Mr. PARKER.

I can easily forgive Dr. Nooth for his representing me as having no other merit than the *first publication* of the method for impregnating water with fixed air, accounting for it as I have done before; but I cannot so easily forgive another paragraph in his paper, the tendency of which is intirely to discredit a method, which, though it is, in some respects, inferior to his own, has nevertheless its peculiar advantages: and every advantage cannot possibly concur in any one method. He says, p. 59, “ Independent of the inconveniencies
“ attending the process, there was another objection to the apparatus, which, with most
“ people, might have considerable weight.
“ The *bladder*, which formed part of it, was
“ thought to render the water offensive; and
“ when the solvent power of fixed air is considered, it will not appear improbable, that

“ the water would be always more or less
“ tainted by the bladder. In some trials which
“ I made with Dr. Priestley’s apparatus, it
“ always happened that the water acquired an
“ *urinous flavour*; and this taste was, in ge-
“ neral, so predominant, that it could not be
“ swallowed without some degree of reluc-
“ tance.”

That Dr. Nooth *did* produce an impregnated water which he could not swallow without reluctance, and even that, in the trials to which he refers, he *generally* produced such water, I am far from doubting; because that might happen from various causes. But that the urinous flavour came from the *bladder*, as such, I will venture to say is not possible. For then it would *always* have had the same effect; and not only myself have never perceived such a flavour as the Doctor complains of, but this is the only complaint of the kind that I have hitherto heard of; though many persons of the most delicate taste, and particularly many ladies, have used the water impregnated in my method for months together. Few persons have had to do with bladders, and fixed air confined in bladders, more than myself; and yet I have never seen any reason to suspect this great *solvent power* of fixed air with respect to
them;

them; especially so as to be apparent in the space of a few minutes.

But supposing the fixed air to be capable of dissolving the whole bladder, and to carry it along with itself into the impregnated water, no physician, or philosopher, will pretend to say that it could have any more tendency to give it an *urinous flavour*, than if it had been any other membrane of the animal body.

Indeed, as the Doctor himself does not pretend to say that this strange urinous flavour was the effect of *all* the impregnations of water made in my method, but only in *some* of them (though it was *generally* so, in those particular trials) it is evident, from his tacit confession, that it must have been an *accidental thing*, and could not have come from the bladder, which I suppose he made use of in all trials. For he has not done me the justice to acknowledge that, in my pamphlet, among the various methods of effecting the impregnation of water, I have described one in which no bladder is made use of. When the Doctor shall once more produce this urinous flavour (and as a new and curious experiment, it is certainly worthy of his farther investigation) taking care that no careless servant shall have mixed any urine in

the water that he calls for, I shall give this new objection to my process a farther examination. At present I am inclined to consider this as an experiment of the servant, rather than of the Doctor himself.

Several persons have thought that fixed air discharged from *impure chalk* gives the water that is impregnated with it a disagreeable flavour; but this I have never observed myself; and any other calcareous matter may be used in my method, as well as in that of Dr. Nooth, who recommends chalk, as the best upon the whole.

I shall conclude these animadversions with doing what Dr. Nooth ought to have done before me, viz. fairly stating the advantages and disadvantages of our two methods. His method requires *less skill* in the operator, and *a less constant attention*. It is also *more elegant* and cleanly, I mean with respect to the *operator*; for this does not at all affect the *impregnated water*. On these accounts I generally recommend and make use of his method myself, especially as the glasses are made with improvements by Mr. Parker. But if Dr. Nooth be candid, he must acknowledge that my method requires much *less time*, and is much *less expensive*; and therefore must be more proper

per when a great quantity of impregnated water is wanted; and especially when there is but little room to make it in.

My method indeed requires a constant attendance, but I question whether, upon the whole, more than is necessary to be given to Dr. Nooth's method at intervals, if the water be at all agitated; considering that mine does not require one-tenth part of the time. And though my method requires some little skill and address, it is not so much, but that many persons, altogether unused to experiments, have, to my knowledge, succeeded in it very well, and have made the impregnated water in a constant way for their family use, and without any assistance besides what they got from the printed directions. My apparatus costs little or nothing, because no vessels are made for the purpose; and both the chalk and the acids are made to go as far as possible, by means of the convenient agitation of the vessel in which they are contained. Whereas Dr. Nooth's method requires a peculiar and expensive apparatus, and more waste is unavoidable in the use of it. However, for the reasons abovementioned, I have never recommended my own method for the use of a family since I have been acquainted with his.

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What I have said above is rather applicable to the apparatus as it is made by Mr. Parker, than to that which Dr. Nooth has described. For Mr. Parker's glasses are, in my opinion, considerably improved from those of Dr. Nooth. It may be said that the improvements consist in *little things*; but little things may have great effects; and, after the discovery of the *first method* of accomplishing this end, all *subsequent methods* may be called little things; and they may be endlessly diversified, without any great claim of merit. I have seen several very ingenious methods since the publication of mine, though none that I like so much, upon the whole, as that of Dr. Nooth, improved by Mr. Parker.

In Dr. Nooth's apparatus, if any more air than is wanted be produced, the water will run out of the uppermost vessel. To use his own words, p. 63, "Should more air be extricated than is sufficient, in the conduct of the process, to fill that vessel, the water will run over the top of it, and will continue to run as long as any air ascends in the middle vessel, or till the surface of the water is below the extremity of the bent tube; and in this case the whole would be wet and disagreeable." But this disagreeable consequence

quence can never happen in the use of Mr. Parker's glasses, because the bent tube in which the uppermost vessel terminates is made of such a length, that the water expelled from the middle vessel can do no more than nearly fill the uppermost, and can never run over; so that whereas Dr. Nooth's apparatus requires a constant attendance, Mr. Parker's requires none. The materials being once put into it, the process will go on of itself, without any farther care; unless the operator should chuse to accelerate the impregnation by now and then letting out the air that is not easily absorbed, and by agitating the water. This I think to be a considerable advantage gained by a very easy contrivance of Mr. Parker's, overlooked by Dr. Nooth.

Mr. Parker derives another considerable advantage from a *channel* which he cuts in the stopper of his uppermost vessel, or from a stopper with a hole through the middle, which Dr. Nooth has not in his; so that either the operator must be careful to take it out during the effervescence, or it will be driven out, or some of the vessels will burst, to the great danger of the by-standers; which actually happened in one made by Mr. Parker, before he thought of this method to prevent it. Whereas,

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through the channel in Mr. Parker's apparatus; the common air easily escapes from the uppermost vessel, to make room for the water to ascend; and when, in the continuance of the process, the fixed air rises through the bent tube into the uppermost vessel, it lodges upon the surface of the water in it; and the communication between it and the common air being so much obstructed, they are sufficiently separated; so that even the water in the uppermost vessel has (if the production of air be copious) almost as much advantage for receiving the impregnation; as that in the middle vessel. This advantage Dr. Nooth loses.

Also, when he chuses to separate the two uppermost vessels from the lowest, in order to agitate the water, he must either leave the mouth of the uppermost vessel open, in which case he can hardly agitate the water at all; or (as he prefers to do it) he must put the stopper in, and consequently admit the common air to pass his valve, and mix with the fixed air, which must greatly retard the absorption of it: whereas Mr. Parker's vessels may be agitated with the stopper in, which, admitting the common air into the upper vessel, through the channel cut in it (or through the hole of the stopper) permits the water to descend into the lower, on
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the surface of which nothing but fixed air is incumbent. Should any common air enter by the valve, which in this case it hardly would, the finger of the person who shakes the vessel may easily be placed so as to prevent it.

Lastly, I consider it as a valuable improvement in Mr. Parker's apparatus, that, by means of the openings into the middle and lowest vessels, closed with ground stopples, the operator is enabled to draw off his water, in order to taste it occasionally, or to add to his oil of vitriol or chalk, &c. at pleasure, without giving himself the trouble of separating the vessels from one another for those purposes.

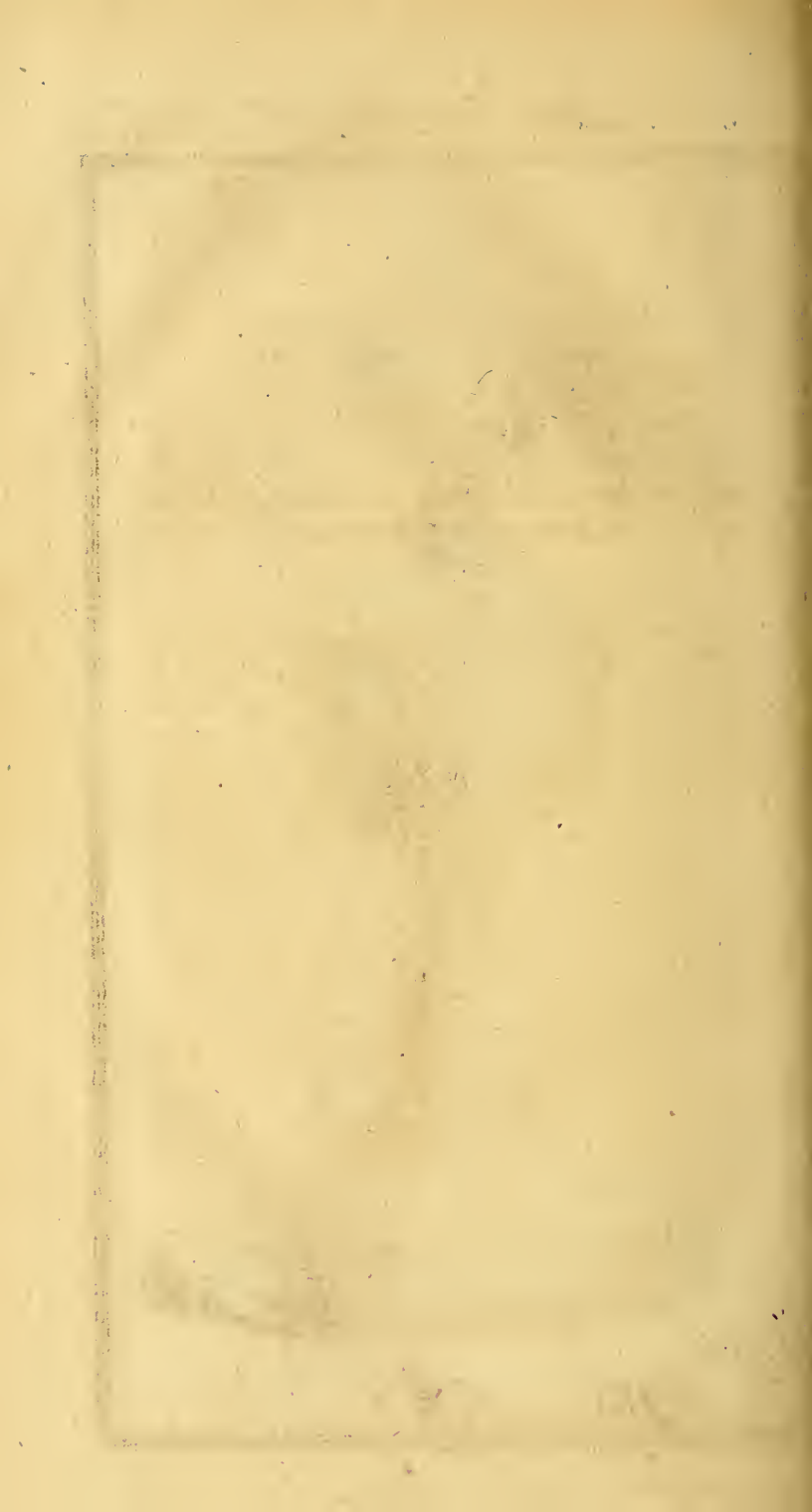
The first apparatus that I saw of Mr. Parker's had no *valve* at all, but only a glass stopple, with one or more small perforations, for the ascent of the air into the middle vessel. This I still generally make use of, without finding any occasion for a valve; the ascent of the fixed air sufficiently preventing the descent of the water, as long as the process continues, especially when pounded *marble* is used. This substance Dr. Franklin recommended to me, and I give it the preference very greatly to chalk, chiefly on account of the length of time that is required to expel the
air

air from it: For without any fresh acid, it will often continue to yield air for several days together.

That those persons who are not possessed of the English *Philosophical Transactions*, and particularly foreigners, may understand what has preceded, I shall in the third plate, in this volume, give a drawing of Dr. Nooth's apparatus, as improved by Mr. Parker, with the following general description of it.

In the lowest vessel, the chalk or marble, and the water acidulated with oil of vitriol, must be put, and into the middle vessel the water to be impregnated. During the effervescence, the fixed air rises into the middle vessel, and rests upon the surface of the water in it, while the water that is displaced by the air rises through the bent tube into the uppermost vessel, the common air going out through the channel in the stopple. When the bent tube is of a proper length, the process requires no attention; and if the production of air be copious, the water will generally be sufficiently impregnated in five or six hours. At least, all the attention that needs be given to it is to raise the uppermost vessel once or twice, to let out that part of the fixed air
which





which is not readily absorbed by water. If the operator chuse to accelerate the process, by agitating the water, he must separate the two uppermost vessels from the lowest. For if he should agitate them all together, he will occasion too copious a production of air; and he will also be in danger of throwing the liquor contained in the lowest vessel into contact with the stopple which separates it from the middle vessel, by which means some of the oil of vitriol might get into the water,

S E C.

SECTION XVI.

An Account of some Misrepresentations of the Author's Sentiments, and of some Differences of Opinion with respect to the Subject of Air.

I have always flattered myself, and the opinion of others has concurred to confirm me in the persuasion, that my writings were very intelligible, so that few persons could well mistake my meaning; and indeed I have no reason to complain of my *countrymen* in this respect. But I have been singularly unfortunate with respect to *foreigners*; owing, I suppose, to their not understanding the English language. For it cannot be that philosophers, and those whom I consider as my fellow-labourers in these researches, should have given so little attention to this business, as to have misrepresented my meaning so grossly as they have done, either through a hasty perusal of my writings, or such an ignorance of the subject, as rendered them incapable of understanding me; much less can it be supposed that any of them would wilfully misrepresent my meaning.

Such, however, is the fact, that, I believe no example can be produced of any person whose meaning has been so egregiously mistaken as mine has been, and even by philosophers, and writers of great reputation, whose works will necessarily go into many hands, and consequently give a very unjust and very unfavourable idea of my sentiments. I think proper, therefore, in this section, to enumerate, in as brief a manner as I possibly can, not *all* the mistakes that have been made by all those who have undertaken to give an account of my experiments, for then I must have made a book upon the subject, but those of a few writers of reputation.

That I do not exaggerate in what I have said above, will not be thought incredible by my reader, when I inform him that in Mr. Roffier's translation of my first papers, communicated to the Royal Society, which made no more than a quarto pamphlet, the French translator of the former volume of this work told me (for I have never had patience to read it myself) that he had noted fourscore faults which affected the sense, exclusive of inaccuracies of style. These fourscore faults I shall therefore intirely pass over; having, I hope, said enough to caution my reader not to look into that work for an account of any thing that I have said or done. I shall not even think it

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worth while to note all the mistakes of Mr. Lavoisier, and I shall be as concise as possible in my remarks, exhibiting what I have been represented as saying in one column, and what I have really said in another. Mr. Lavoisier's work is intitled, *Opuscles Physiques & Chymiques*, and will soon be published in English; Sig. Landriani's is called *Ricerche Fisiche intorno alla salubrità dell'aria*.

Mr. Lavoisier's Account of my Experiments and Observations.

The true Account of them.

Monf. Priestley, p. 111, asserts a fact which would prove that fixed air is not heavier than common air; saying that a candle will continue to burn in a vessel plunged into an atmosphere of fixed air with its mouth *upwards*.

On the contrary, I said, p. 27, that the vessel had its mouth *downwards*; from which a contrary conclusion will follow.

P. 112, he says that gunpowder has this *peculiar property*, that being fired in fixed air, the whole of it will incorporate with that air, and that no part of it will escape into the common air.

I have only said that this would be the case when the quantity of gunpowder was very small, and the body of fixed air on the surface of the fermenting liquor very strong.

P. 114, Monf. Priestley asserts as an extraordinary fact, that fixed air diminished by iron filings and brimstone is not noxious to animals, and that it does not differ from common air.

What I have said, p. 42, is that, *in one case only*, a quantity of this air was not very noxious to animals; and I attribute that degree of wholesomeness in this air to my having inadvertently agitated it in water, at a time when I was not aware of the effect of

such agitation; and in the same place I observe, that in another quantity of fixed air, which had undergone the same process, a mouse died pretty soon.

P. 115, One of his experiments would seem to prove that there is an acid in fixed air, whereas others of his experiments contradict that opinion.

The *other experiments* are those of Mr. Hey, which were not intended to prove that fixed air is not an acid; but that water impregnated with it is not made acid by any of the oil of vitriol being rendered volatile, and mixing with it.

P. 116, Snails die immediately and irrecoverably in fixed air.

I only mentioned a *single experiment*, p. 36, with one particular snail.

P. 119, He transferred very hot air into a receiver, and having placed a candle in it, found that it burned as well as in cold air.

In reality I only put the candle (see p. 49) into air that *had been made very hot*, but which was then quite cold.

P. 122, Inflammable air long agitated in water appears to differ in nothing from common air.

I said, p. 68, that a candle burned in this air as in common air, only more faintly; but that, by the test of nitrous air, it did not appear to be near so good as common air; and that by longer agitation it extinguished a candle.

Ib. Inflammable air from oak has this peculiar quality, that water can absorb one half of it.

I only said, p. 69, that I made a particular experiment with a quantity of this air, after having agitated it in water till it was diminished about one half. How much more it might have been di-

minished, by longer agitation,
I did not say.

P. 123. Inflammable air
does not injure the growth of
plants.

I only said, p. 61, that
plants grew pretty well in one
quantity of inflammable air
that was made from zinc, and
in another from oak.

P. 125, Air injured by re-
spiration approaches to the
state of fixed air, because it
can combine with lime; but
it differs from it, because when
mixed with common air it di-
minishes the bulk of it:
whereas fixed air increases it.
It is also not absorbed by
water, like fixed air.

Air injured by respiration
will not unite with lime in
lime water; though when air
is thus injured something is
precipitated from it, which has
that effect. I have no where
said that air injured by respi-
ration diminishes the quantity
of common air with which it
is mixed, though that *princi-
ple* which had diminished this
air will diminish any other
wholesome air. Mr. Lavoisier
himself quotes me, p. 129, as
saying, that when I had mixed
air injured by putrefaction
with common air, the quan-
tity was *not* diminished. If
this injured air is not capable
of being absorbed by water,
which is the case, it must differ
very essentially from fixed air.

P. 130, He has made a
great number of experiments,
which shew that plants vege-
tating in corrupted air make
it as proper for respiration as
the air of the atmosphere.

I never pretended to have
restored air that was *thoroughly
noxious* by any method, so far
as that a candle would burn
in it. Though the growth of
plants in air which had never
been more injured than that
a candle would go out in it,
(which is a much greater in-
jury than the general mass of
the atmosphere ever suffers)

never failed to restore it so far as that a candle would burn in it, to all appearance, as well as ever,

P. 131, Monf. Priestley says, that all kinds of noxious air are restored by long agitation in water; which seems to contradict what he says in another place, that agitation in water was not sufficient to deprive noxious air of its deadly quality.

I said, in an account of an experiment in which I frequently transferred a quantity of noxious air from one vessel to another, that I did not find it to be restored by that means. But this is a very different operation from the agitation of air in water, and especially when it is continued a long time.

P. 136, Monf. Priestley says, that nitrous air may be absorbed by water; but with respect to the quantity of absorption, the results of his experiments do not seem to agree exactly.

I have no where stated the *ulimum* of the absorption of nitrous air by water. Indeed, strictly speaking, water deprived of all air will absorb the whole of any kind of air. I only mentioned different degrees of absorption, as I observed them in circumstances that were considerably different.

P. 137, A paste of iron filings and brimstone diminishes common air one-fourth: but by means of nitrous air this diminution may be carried much farther. For if a quantity of nitrous air be admitted to this mixture, the common air will be reduced to one-fourth of its bulk.

What I have said is, that the *nitrous air* itself, and not *common air*, was diminished to one-fourth of its bulk by the fermentation of that paste. No common air was concerned in the experiment.

P. 140, By throwing the focus of a burning mirror on a piece of charcoal in com-

The fixed air in this experiment, in which there was so great a diminution of the

mon air, it was diminished one-fifth, and the remainder was partly fixed air, and partly inflammable.

common air, must have been chiefly, if not wholly, that which was precipitated from the common air. The remainder was so far from being inflammable, that it extinguished a candle.

Ib. If the charcoal be made with a very hot fire, capable of melting the crucible, there will be no sensible diminution of the air in which it is heated. Charcoal that has been moderately calcined gives no sign of inflammable air.

This was the case in one experiment, but it only proves that *in some cases* air is not so easily expelled from charcoal as in others. But I have always represented the very contrary of these results as true in general, viz. that a longer continuance of heat, and a greater degree of it, expels more air from wood, and that afterwards the purer will be the phlogiston that is expelled from it, and consequently the greater probability there is that the air in which it is heated will be diminished, and not increased.

Ib. If the abovementioned process be made over quicksilver, and not over water, the air will not be diminished.

This is only the case when fixed or inflammable air has been set loose from the charcoal in the process.

P. 141, The piece of charcoal employed in this experiment weighed exactly twenty-nine grains.

It weighed exactly two grains, p. 132.

Mr. Lavoisier, as an introduction to the account of his own experiments, in the work abovementioned, has undertaken to give a pretty full account of all that had been done before him in the same way. I hope he has been more exact with respect to others, than he has been with respect to me,

Sig. Landriani's Account of my
Experiments and Observations.

Remarks.

This writer takes it for granted, through the whole of his work (see p. 6, of the Introduction, and p. 3, of the work itself) that I consider the fixed air in the atmosphere as *un elemento di salubrità*, by which I suppose he means that it is the principle in which its respirability consists, or which makes it fit for respiration.

All that I have said that could lead to this construction is, perhaps that fixed air, though it certainly kills when it is breathed unmixed, does no sensible injury to the lungs when it is mixed with common air; as fire is not noxious *per se*, but only *in excess*. I have also shewn that when common air is made noxious by any phlogistic process, the fixed air contained in it is precipitated. But though this is a circumstance that always attends the corruption of air, I never supposed that the fixed air which it deposits was the *principle of salubrity*. If so, I must have supposed that fixed air mixed with air that had been injured by phlogistic processes might have restored it, which it does not do in the least degree.

P. 24, The diminution of nitrous air by common air, Dr. Priestley supposes, comes from the property that phlogiston has to contract the dimensions of bodies: but he gives no sufficient proof of that supposition; and though he suspects that this contraction is a real levity communicated to the air by phlogiston, he has not ventured absolutely to assert it; though chemistry

I do not recollect that I have any where said that phlogiston contracts the dimensions of bodies. On the contrary, I say that air injured by phlogiston is *specifically lighter* than common air. And what I say of the *principle of levity*, p. 267, is, that it is a supposition I am not willing to have recourse to, though it would afford an easy solution of the difficulty.

furnishes many examples of this singular property of phlogiston.

I might have expressed myself more strongly; for I never had any faith at all in that doctrine of the principle of levity. On the contrary, as may be seen, p. 293, I consider the difference of weight between a metal and a calx, which has given occasion to that doctrine, as wholly owing to the fixed air and water imbibed by the latter in the act of calcination.

P. 31, Sig. Landriani represents me as stating the limit of the diminution of common air by nitrous air to be when *precisely* two measures of the former are mixed with one of the latter; but says that he has found that this depends upon the quality of the two kinds of air, and especially on the quantity of phlogiston contained in the nitrous air.

I have only said, p. 110, that after many trials, I have found that the greatest diminution is when *about* one-third of nitrous air is mixed with common air, which implies that I was aware of a considerable variety in the results of such experiments; and the whole of my narrative shews that I have considered a less diminution to arise sometimes from the common air having already more phlogiston than usual, and sometimes from the nitrous air communicating less. I frequently speak of different quantities of nitrous air as possessing very different powers of diminishing common air, and sometimes speak of nitrous air as reduced to a state in which it had no power of diminishing common air at all. And as I make it a maxim that common air is diminished and made noxious

by phlogiston only, I must necessarily have considered, that nitrous air in this case either as not containing phlogiston, or as not disposed to part with it.

P. 32, Dr. Priestley believes that inflammable air becomes respirable by agitation in water, because part of the phlogiston is deposited in the water, and another part of it remains to sweeten the acid air, and make it respirable.

The former I have supposed, but I do not remember to have said any thing about the latter. I had, indeed, imagined, that acid air and phlogiston composed inflammable air; and supposed part of the phlogiston to be absorbed by water, when it ceases to be inflammable, by means of agitation in water. But I must have supposed, agreeable to the maxim above-mentioned, that the phlogiston which remained must have contributed to keep the air in a worse state than it would be in, if it could be expelled by it, which is the very contrary of what Sig. Landriani has ascribed to me.

Lastly, The most unaccountable mistake concerning any of my opinions relating to air, is that I should be supposed to maintain that *fixed air is a combination of common air and phlogiston*. Mr. Lavoisier in Rosier's Journal for May 1775, p. 433, says, " Since common air is changed into fixed air by a combination with charcoal, it may seem natural to conclude that fixed air " is

“ is nothing but a combination of common
“ air and phlogiston. This opinion is that of
“ Monf. Priestley.”

This, indeed, is the opinion of Dr. Rutherford (*Dissertatio de aire fixo*, p. 25.) of an English Chymist, who probably had it from him, and that of other philosophers in this country, who may have adopted it from them; but every thing that is English is not mine. I have mistakes enow of my own to answer for, and I cannot conceive how any thing that I have ever advanced on the subject should have been construed to bear that meaning.

The proposition which Mr. Lavoisier advances in the preceding paragraph, on which he supposes the hypothesis which he ascribes to me to have been founded, is not true. I do not know that common air can, by any process, be changed into fixed air; and so far am I from supposing that fixed air is a compound of common air and phlogiston, that, on the other hand, I have always rather considered fixed air as an elementary substance, and common air as a compound. Moreover, having brought fixed air, by a supposed union with phlogiston, to be immiscible in water, and to have some of the properties of common air, I was then inclined to think that fixed air
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and phlogiston might make common air, which is the very reverse of the opinion that Mr. Lavoisier ascribes to me. And I do not know that I have ever advanced any thing that comes nearer to that opinion, than this which is expressly contrary to it.

In the report made to the Royal Academy of Sciences concerning the abovementioned treatise of Mr. Lavoisier's by Mr. De Trudaine, Mr. Macquer, Mr. Cadet, and the Secretary M. De Fouchy, these gentlemen say, " Monsr. Priestley considers fixed air as very nearly of the same specific gravity with the air of the atmosphere." Now I have always considered fixed air as *considerably heavier* than common air. Indeed, I never made any observation of my own on that subject, having only adopted the conclusion of Mr. Cavendish, whose discovery it was.

I imagine these gentlemen have mistaken what I have said concerning *air injured air by phlogiston*, which I have said was very nearly of the same specific gravity with common air, though rather lighter, for what I have said concerning *fixed air*. And it is possible that these gentlemen, like Mr. Lavoisier, may have taken it for granted that these two kinds of air
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are the same; though I always speak of them as very different things.

As Dr. Rutherford, when he published his *Dissertation on fixed air*, had only heard of my experiments, it would not be worth while to take notice of his mistakes concerning them; but his treatise being translated into French, in Mr. Rosier's Journal, I shall just observe, now that I am upon the subject, that he also supposes, p. 25, I had restored *fixed air* to a fitness for respiration by vegetation, whereas it was *air injured by respiration or putrefaction*. But this author, as well as many others, makes no difference between these two very different kinds of air.

No person can lay a less stress upon *opinions*, and more upon *facts*, than I have done, in all my philosophical writings; and the opinions I have advanced are very few in proportion to the new and important facts that I have discovered. I therefore think it rather hard, that those very few opinions should have been so grossly misrepresented as they have been.

All the doubts that I have ever entertained with respect to the constitution of fixed air have little relation to the differences of opinion maintained by others concerning it. I was always
inclined

inclined to think fixed air to be an acid *sui generis*, as much as any of the three mineral acids. But as it is the opinion of several of the most eminent chymists, that even these three acids, as well as all other acids, are only one and the same acid, differently modified and combined, and that they are therefore transmutable into one another, I have of late conjectured that the fixed air which I have sometimes produced must have been a transmutation of the nitrous acid into it, because no substance employed in the experiments can well be thought to have contained the fixed air, notwithstanding the air that was produced discovered the most undeniable signs of it, as will have been seen in the preceding narrative. Still, however, I do not pretend to have formed any decisive opinion upon the subject. Let the facts be considered, and speak for themselves.

It is maintained by Sig. Landriani, whose treatise I did not receive till a considerable part of this volume was printed off, that fixed air is of a different constitution according to the acid by which it is procured from calcareous substances. He says, among other things, p. 48, that the salt which is formed by the union of alkaline air with fixed air by oil of vitriol

vitriol is a true vitriolic ammoniac, and that the salt which is formed by alkaline air and the air which is procured by means of spirit of nitre has the property of detonating by itself, which is known to be peculiar to the nitrous ammoniac. He also says that fixed air procured by the *vegetable* acids has not the same power of reddening the juice of turnsole with that which is procured by the *mineral* acids. These are very remarkable experiments, and deserve to be repeated, and considered with attention. They have led our author to conclude, that all the different kinds of air are, in fact, one and the same thing, which has the property of holding in solution various bodies, and particularly the acids, see p. 33. Accordingly nitrous air, in his opinion, is common air holding in solution a quantity of the nitrous acid overcharged with phlogiston.

That excellent philosopher Felice Fontana, in his *Ricerche Fisiche sopra l'aria fissa*, maintains, that all the acidity of fixed air comes from the oil of vitriol dissolved in it, and which is so intimately united to it, as to be afterwards inseparable from it; insomuch that when it has been incorporated with water, and expelled from it again, it carries away all
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the acid vapour along with it; having all the same properties that it had before it was combined with the water. The acid of vitriol; thus attenuated and exalted, by its solution in fixed air, is more penetrating, he says, and has more medicinal virtues, than the same acid dissolved in water, or administered in any other form.

Fixed air deprived of this acidity, which is foreign to its nature, he supposes to be the same thing with atmospherical air deprived of its peculiar acid by phlogistic processes, an acid which he maintains to be altogether different from any acid with which we are acquainted, and which he proposes to investigate; conceiving this acid to be the great principle of salubrity in the atmosphere. This writer says, that he has attempted in vain to make water acidulous by means of fixed air expelled from substances without the help of other acids, as in putrefaction. But he does not appear to have tried what he could have done with calcareous substances by heat only.

I take this early opportunity of publishing the sentiments of so considerable a person, though it will appear that they are very different from my own, in order to promote a farther investigation of the subject.

Having mentioned the paper of Mr. Lavoisier's, published in Mr. Rosier's Journal; I would observe, that it appears by it, that, after I left Paris, where I procured the *mercurius calcinatus* abovementioned, and had spoken of the experiments that I had made; and that I intended to make with it, he began his experiments upon the same substance, and presently found what I have called *dephlogisticated air*, but without investigating the nature of it; and indeed, without being fully apprised of the degree of its purity. For he had only tried it with one-third of nitrous air, and observed that a candle burned in it with more vigour than in common air; and though he says it *seems to be* more fit for respiration than common air, he does not say that he had made any trial how long an animal could live in it.

He therefore inferred, as I have said that I myself had once done, that this substance had, during the process of calcination, imbibed atmospherical air, not in part, but in whole. But then he extends his conclusion; and, as it appears to me, without any evidence, to all the metallic calces; saying that, very probably, they would all of them yield only common air, if, like *mercurius calcinatus*, they could be reduced without addition. For he

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considers the fixed air, which is yielded by most of them, to come from the charcoal, made use of to revivify the calx. Whereas it will be seen, in the course of my experiments, that several of those calces yield fixed air by *heat only*, without any addition of charcoal.

He adds, that since common air is changed into fixed air when it is combined with charcoal, it would seem natural to conclude, that fixed air is only a combination of common air and phlogiston (an opinion which, as has been seen before, he ascribes to me) and it is not, he says, without probability; but adds, that it is so often contradicted by facts, that he desires philosophers and chymists to suspend their judgments; hoping that it will soon be in his power to explain the motives of his doubts. I, for one, am waiting with some impatience for this explanation.

Mr. Lavoisier also concludes, from his observations, that the air produced by the detonation of nitre and the firing of gunpowder is common air. When he sees this volume of mine, he will, I doubt not, be convinced of the imperfection of his theory, and of this mistake, which he has been led into by means of it.

Y

Mr.

Mr. Lavoisier, as well as Sig. Landriani, Sig. F. Fontana, and indeed all other writers except myself, seems to consider common air (divested of the effluvia that float in it, and various substances that are dissolved in it, but which are in reality foreign to it) as a simple *elementary body*; whereas I have, for a long time, considered it as a *compound*; and this notion has been of great service to me in my inquiries.

As a concurrence of unforeseen and undesigned circumstances has favoured me in this inquiry, a like happy concurrence may favour Mr. Lavoisier in another; and as, in this case, truth has been the means of leading him into error, error may, in its turn, lead him into truth. It will have been seen, in the course of my writings, that both these circumstances have frequently happened to myself; and indeed examples of both of them will be found in my first section concerning this very subject of dephlogisticated air.

It is pleasant when we can be equally amused with our own mistakes, and those of others. I have voluntarily given others many opportunities of amusing themselves with mine, when it was entirely in my power to have concealed

cealed them. But I was determined to shew how little *mystery* there really is in the business of experimental philosophy, and with how little *sagacity*, or even *design*, discoveries (which some persons are pleased to consider as great and wonderful things) have been made.

SECTION XVII.

Experiments relating to some of the preceding Sections made since they were printed off.

Having had an opportunity of making a few farther experiments relating to some kinds of air mentioned in this treatise, after the sections relating to them were printed off, I have thought it would be better to subjoin an account of them in this place, rather than defer it to another publication.

1. Of the vitriolic and vegetable acid airs.

It will be seen, by comparing the first and second sections of this volume, that there is a remarkable resemblance between the vitriolic and vegetable acid airs, and I have since observed other circumstances of resemblance.

The electric spark taken in vegetable acid air produces the very same effect as in the vitriolic acid air, tinging the glass tube in which it is contained with a deep brown, or black colour. I took about fifty explosions of a common jar in a small quantity of it, after which water imbibed almost the whole of it.

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It is remarkable that the glass becomes almost as deeply tinged as it can be made by the experiment, after a very few of the explosions.

I also observed the same remarkable effect of putting small glass tubes half filled with water into vitriolic acid air, that I have described p. 26, as observed in the vegetable acid air, viz. that if a little air be left at the bottom of the tube, it will swell, and drive out all the water. The reason of this appearance I believe to be, that the water, being presently saturated with this acid air, transmits it to the common air in the tube; which, receiving a continual increase of bulk from this source, at length expels all the water.

I observed this appearance when I put the tubes, thus partially filled with water, into that air which I had expelled from the water that had been saturated with the fluor acid air; which is another argument of the identity of this acid with the vitriolic.

Water is soon impregnated with vitriolic acid air, but has little power of retaining it; so that the smell of the water so impregnated is the most pungent that can be conceived, and if it stands exposed to the common air, the acid air, in a great measure, presently

quits it. Also the least agitation of the water promotes the separation of the air from it.

I have observed, p. 10, that a mixture of vitriolic acid air injures common air, and that the effluvium of the concentrated vegetable acid has the same effect. I have since found that a mixture of the vegetable acid air itself does so to. Two measures of this mixture, and one of nitrous air occupied the space of two measures.

The only real difference between the vegetable and the vitriolic acid air (besides the *smell* of them, in which respect the difference is remarkable enough) that I have observed, is, that, whereas the vitriolic acid air, as well as all the other acid airs with which I am acquainted, deepens the colour of olive oil, an impregnation with vegetable acid air makes it more colourless. In one experiment, however, vegetable acid air gave a yellowish tinge to oil of turpentine, which is an effect that vitriolic acid air has upon it; though, upon another occasion, the result of this experiment was different, and I have not leisure at present to examine whence this difference arose.

As Dr. Higgins has informed me, that oil of vitriol was employed in preparing the concentrated

centrated vinegar that I made use of for the production of vegetable acid air, I think it possible that the air which I expelled from it may have been, in part, of the vitriolic kind; but I do not know of any other vegetable acid liquor that will yield air; at least in a quantity sufficient for any experiments. I tried *radical vinegar of the crystals of verdigris rectified*, which was recommended to me, and made for me, by Mr. Woulfe, and also concentrated acid made from *sal diureticus*, by Mr. Godfrey; but neither of these acid liquors, though the smell of them was extremely pungent, yielded any air by heat.

The common air expelled from the phial by the steam of this vinegar, mixed with whatever acid vapour might come over along with it, I examined, after letting it rest upon quicksilver a whole night, and I found it not to differ from common air.

When, however, I tried this experiment with air that had lodged on the surface of oil of vitriol, into which I had put some *sal diureticus*, and which did yield a little air, the common air did appear to be injured by the mixture, as in the preceding experiment of the mixture of common and vegetable acid air. But then oil of vitriol being

employed in this experiment, as well as in the preparation of the concentrated vinegar above-mentioned, it is liable to the same objection; the acid of vitriol being, perhaps, volatilized by some small portion of phlogiston.

2. *Of dephlogisticated air.*

I have observed a great variety in the results of the experiments for the production of dephlogisticated air, both with respect to the quantity, and the quality of it, especially as mixed with a greater or less proportion of fixed air. From the following experiments it will appear that the quantity of dephlogisticated air depends upon the quantity of the spirit of nitre made use of in the process, the quantity of fixed air being nearly the same in all the cases.

From an ounce of red lead, heated in a gun-barrel, I got about an ounce measure of air, which all together was worse than common air; an effect which I attribute, in a great measure, to phlogiston discharged from the iron. The production of air in this case was very slow.

From an ounce measure of the same red lead, diluted with half spirit of nitre and half
water,

water, I got twelve ounce measures of air, the last produce of which was highly nitrous. Half of this quantity was absorbed by water, and the remainder was twice as good as common air.

From an ounce of the same red lead, diluted with the same spirit of nitre, without water, I got, by the same treatment, about thirty ounce measures of air, about one-eighth of which was absorbed by water, while the rest was highly dephlogisticated.

From the same quantity of red lead, moistened with twice the quantity of the same spirit of nitre, I got about sixty ounce measures of air, a very small part of which was absorbed by water, and the rest was as highly dephlogisticated as that in the last experiment.

The produce of air was quicker, with the same degree of heat, in proportion as the quantity produced was greater; and in the last process the air was very red in the inside of the vessel that received it, for a considerable time.

3. *Of the effect of the nitrous acid on common air.*

I have shewn, in a variety of experiments, that the fumes of spirit of nitre injure common air. I have found the same to be the effect of the effluvia of *nitrous ether*. For the air which had been confined about a week, in a bottle in which a quantity of nitrous ether had been kept, was so much injured, that two measures of it, and one of nitrous air, occupied the space of $2\frac{1}{2}$ measures. As I let a good deal of common air into the phial, at the same time that (not chusing to lose it) I poured the ether out of it into another phial, I conclude that the air in the phial was almost perfectly noxious.

I have more than once expressed an earnest wish that I could meet with any fluid substance that was not affected with the nitrous acid, as this would give me an opportunity of confining the nitrous acid air, in order to make experiments upon it, as I have done upon other acid airs; and I almost flatter myself that I have accidentally met with one that will answer my purpose. It will be seen, p. 156, that *hog's lard* is very little affected with boiling spirit of nitre. Upon finding this, I immediately endeavoured, by means of heat, to expel

expel nitrous acid air from a quantity of strong spirit of nitre; thinking that it might be confined in a vessel filled with melted hog's lard, as the other acid airs had been confined by quicksilver. But though I made the spirit of nitre boil a long time, I got nothing from it but the common air which had lodged on the surface of the acid, and which I found to be so far injured by the process, that two measures of it, and one of nitrous air, occupied the space of $2\frac{1}{2}$ measures. I shall try whether I cannot have better success with some other *animal oil*, as the *spermaceti oil*, &c. making some other varieties in the process.

4. Of fixed air.

I have made an observation, p. 220, of the degree of the purity of the residuum of fixed air which had been wholly contained in water, at a time when it hardly made lime water turbid. I afterwards kept the same residuum, washing it several times in lime water, till it had no effect upon it whatever. At this time two measures of it, and one of nitrous air, occupied the space of $2\frac{1}{2}$ measures. In fact, therefore, the residuum of fixed air is, in the main, the same thing with phlogisticated common air; though in this case it was meliorated by so much washing in water. To the
same

same state also are all kinds of air whatever, and even nitrous air itself, reduced, by much agitation in water. This is a remarkable fact, and may furnish matter for speculation.

To my short account of my observations on the Seltzer spring, and the other mineral water near Mentz, p. 226, I would add, that the bottom of both of them, and also of the current of water that ran from them, was tinged red with ochre, so that it is evident they both contain iron.

5. *Of the impregnation of water with fixed air.*

I find I have expressed myself too strongly with respect to the evidence of other persons having had in view any scheme of the impregnation of water with fixed air, before the publication of my pamphlet on that subject; on the supposition that, if such evidence had existed, it would have found its way to the public by this time.

My ingenious correspondent Mr. Bewly, on seeing that part of the work, informs me, that he had not read Dr. Brownrigg's paper half through, before he expected that the *synthesis* would follow the *analysis*, and that finding his author intirely silent on the subject, he immediately

diately went to work himself, and in a common phial effected the impregnation, by fixed air set loose from salt of tartar; and though he had but an imperfect kind of an apparatus, he says he has occasionally regaled himself and his friends with small potations of artificial Pymont water, ever since the publication of Dr. Brownrigg's paper.

I cannot help observing on this occasion, as on many others, that it is much to be regretted, that persons of a philosophical turn should not be more disposed to communicate their discoveries to the public. In this case, however, it will be seen that I am not myself without blame, as I made no publication on the subject till some years after I had effected this impregnation, though Mr. Bewly, I find, had done the same thing a considerable time before me.

6. *Of the use of terms.*

I am sorry to find, that notwithstanding what I said, in the preface to my former volume, on my choice of the term *air*, as applied to the *nitrous*, *acid*, and *alkaline* principles exhibited in that form, some persons are either so weak, or so captious, as not to be satisfied.

No

No person was ever more temperate, or more cautious, than I have been in the introduction of *new terms*, considering the number of *new facts* that I have discovered. It was with great hesitation, though compelled by necessity, that I did it at all, generally with the advice of my most judicious friends, and always adopting such as were analogous to others in established use. Thus when I found the terms *common or atmospherical air*, *fixed air*, and *inflammable air*; used by all philosophers, and no person whatever had objected to them, it was certainly natural for me to continue to apply the term *air* to other *elastic transparent fluids*, not condensable by cold, and to distinguish them by other appellations, drawn from the peculiar circumstances of their production, as *nitrous air*, *acid air*, *alkaline air*, *phlogisticated* and *dephlogisticated air*; using the term *air* as expressive of the mere *form* in which a substance is exhibited, without any consideration of its being simple or compound.

They who chuse to apply the term *air* to a *substance*, and not to a *form*, are certainly at full liberty so to do, if they please; and provided we understand one another, no inconvenience will result from our use of a different language. But then the same persons should be uniform in their objections and practice,
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and call nothing by the name of *air* that they do not believe to consist of that one *elementary substance* to which they profess to appropriate the term. I will add also, that such persons will do well to prove that there *is* such an elementary substance, and to reconcile the facts that I have discovered with that hypothesis. The language that I adopt implies no attachment to any hypothesis whatever, and may still be used though I should change my opinion on that subject; which is certainly a very great advantage in philosophical language.

T H E

THE
APPENDIX.

NUMBER I.

EXPERIMENTS and OBSERVATIONS *relating to some of the Chemical Properties of the Fluid, commonly called FIXED AIR; and tending to prove that it is merely the VAPOUR of a particular ACID. In two Letters to the Reverend Dr. Priestley: By William Bewly.*

ANY successful investigation of that part of philosophy, in which you have lately made so extensive and rapid a progress, cannot be communicated to the publick any where with so much propriety, and advantage, as in the company of those singular and important discoveries which will be given in the new Volume of your *Observations*, now in the press. With great pleasure, therefore, I comply with your late request, to transmit to you the particulars of my Observations on *Mephitic* or *Fixed Air*; the general results of which I formerly communicated to you. They tend, if I do not deceive myself, to throw a new and just light on the real nature and chemical properties of that fluid; the extensive diffusion of which throughout the universe, where it forms a constituent principle of almost all known bodies, renders it a subject deserving of an accurate and minute investigation.

In the present letter I shall principally confine myself to those Observations only which first convinced me of the existence of an acid *in fixed air*:—a point which has been contested, or, at least, left dubious,
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by other inquirers. The experiments which I shall relate in a subsequent letter will, I expect, satisfactorily evince, that this acid is not a substance extrinsic to fixed air, or casually floating in it, and separable from it; but, on the contrary, that it is a necessarily constituent principle of this fluid; and even that fixed air itself is no other than this very acid; or, in other words, that it is a peculiar and distinct acid spirit, *sui generis*, which, on its being expelled, by the power of a superior acid, or the force of fire, from the various earths, salts, &c. with which it is combined, instantly assumes the form of an elastic vapour, greatly resembling common air; which form it permanently retains, till it meets with any of those numerous bodies which have an affinity to it, and which have been deprived of, or are not already saturated with it. By these bodies this acid vapour is *condensed*, or reduced into a *liquid* or fixed state; in which state it combines with them, in a manner in no respect different from that in which the vitriolic or any other acid is united with the various salts, earths, or other substances, with which they form neutral compounds—Such, at least, is the system which I have been naturally led to deduce from the following experiments.

Even the bare presence of an acid, *in* fixed air, has, as I have already observed, been doubted of. So lately even as the last year Dr. Brownrigg *, to whom this new branch of Chemical philosophy is so highly indebted, observed, that though Mephitic air, imparts to the waters impregnated with it a brisk and pungent taste, which has usually been stiled *subacid*; yet it differs from all acid spirits in not striking a red colour

* Phil. Transf. vol. 64. part 2. for the year 1774. p. 369:
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with the blue tinctures of vegetables ; adding, that not only no change of this kind could be observed to have been effected by it, in the numerous experiments made by himself and several other gentlemen ; but likewise that he had “ for several days suspended “ pieces of linen, that had been dyed blue with fresh “ juice of violets, in the mephitic air of *Spa* water, “ and also in that of chalk ; and when the linen was “ taken out of the said air, did not perceive its blue “ colour in any wise changed, although the same “ pieces of dyed linen were instantly turned of a green “ colour, when exposed to the fumes of the spirit of “ hartshorn.”——“ Whether therefore,” he adds, “ and under what relations, this aerio-saline spirit “ may merit the title of an *acid*, I leave to the de- “ termination of others.”

In the *Appendix* to your former Volume, your ingenious correspondent, Mr. Hey †, has likewise shewn that water impregnated with fixed air, produced no change of colour in the syrup of violets ; and that it did not effervesce with either the fixed or the volatile alkali.——The fact is, that *fixed air* is so rare a *vapour*, and the *Mephitic Acid*, as I shall already venture to call it, is so greatly diluted in water, which is even saturated with it, that many of the blue juices resist its action upon them ; while others, more sensible tests of acidity (such as infusions of Litmus, *Cyanus*, or Corn-flower, and a few others) readily announce its acid quality.——As to its not producing an effervescence with alcalis, it will appear from the following experiments, that such effervescence is, from the very nature of the thing, impossible in the present

† Experiments and Observations, &c. p. 288. 1st edit.

case, in which the very contrary of an effervescence must take place. In all other cases, when an acid is added to a mild alkali, the *mephitic acid*, as, being the least powerful of all the acids, is *expelled*, in its state of *vapour*, or in elastic bubbles, which constitute the appearance called an effervescence; whereas when the mephitic acid itself is added to an alkali, it is *condensed*, and silently absorbed in it.

It may be necessary to premise that, in several of the following experiments, I found it most convenient, as well as productive of greater accuracy and expedition, to take the inverted phial out of the basin, after every fresh introduction of fixed air, for the purpose of agitating more freely the liquor contained in it; and that I took care to use a basin or cup of a very small diameter, and which contained a very small quantity of fluid; in order to guard, as much as possible, against dissipation of the fixed air, during the process. It may be proper likewise to observe, that I may not incur a suspicion of plagiarism, that some scattered hints, relative to a few of the following Observations, have been formerly inserted by me in a certain anonymous publication.

The experiment, by which I first detected the presence of an acid in fixed air, some years ago, is as follows. I have repeated and diversified it on the present occasion, and with the same event.

EXPERIMENT I.

Having accurately adapted, to the mouth of a phial containing spirit of vitriol, a cork, in which a glass tube was inserted, which was drawn out at its farther extremity, so as to terminate in a bore nearly capillary;

lary; and having thrown into it some salt of tartar, I hastily applied the cork, and instantly presented, close to the end of the tube, a piece of a particular kind of blue paper, used for the covers of pamphlets, as well as other pieces of paper tinged blue with the scrapings of radishes. When the effervescence was brisk, and proper expedition was used, the mephitic vapour rushing out, undiluted with common air, and in a dense and sometimes visible column, instantly changed those parts of the blue paper, towards which it was directed, of a bright red colour. On bringing the tongue likewise to the end of the tube, the sensation of acidity was very sensible.

EXPERIMENT II.

The success of the preceding experiment wholly depends on the density and velocity of the mephitic blast. Having afterwards caused the fixed air to pass through moist alkaline salt introduced into the tube, it now only, in general, weakened or discharged the colour of the blue paper. This effect I was at first inclined to attribute to the vitriolic acid, phlogisticated or volatilised, which is known to act in this manner on various coloured substances: but from the following experiments it may be inferred, that the change was produced by the mephitic acid's being in part neutralised, and consequently diminished in quantity, in its passage through the alkali; so that the remaining vapour, though as acid as before, was in too rare a state, and had not *momentum* sufficient to produce the red colour.

EXPERIMENT III.

Six ounces of a weak infusion of Litmus in water, being impregnated with two or three ounces of fixed air, had its blue changed to a red or pink colour.—A weak and nearly colourless infusion of the petals of the corn-flower, as well as infusions of two or three other blue field flowers, acquired likewise a slight reddish tinge, on being even weakly impregnated with fixed air.

EXPERIMENT IV.

Having prepared a *Hepar Sulphuris*, in the liquid way, and in which the alkali was fully saturated with the sulphur; I diluted a part of it with rain water, and added to it a few ounces of water saturated with fixed air. The impregnated water produced the effects which are known to follow the addition of any acid to an alkaline solution of sulphur. The liquor became milky and opaque; and after some time part of the sulphur was precipitated: doubtless by the action of the mephitic acid, which joined itself to the alkali, and thereby disengaged a proportional part of the sulphur before combined with it.

EXPERIMENT V.

The successive action of the acid in fixed air on such of the blue vegetable juices as it changes to red, or its gradual entrance into water, is very pleasingly exhibited by filling a phial, which has a small hole drilled near its bottom, with an infusion of Litmus, and then introducing into its neck a perforated
I cork,

cork, to which is fixed a bladder, containing fixed air. Pressing the bladder till the liquor descends to the broad part of the phial, the perforation is to be stopped; and the infusion being suffered to remain perfectly at rest, the gradual entrance of the fixed air into it (or rather the condensation of the mephitic acid) will be rendered visible, by the successive change of colour in the liquor, from the surface downwards, from blue to red.—This experiment may be diversified by employing the processes indicated in Experiment 7th.

The fixed air employed in the preceding experiments was generally procured from salt of tartar, by means of the vitriolic acid. The greater part however of these and the following experiments were repeated with fixed air, obtained from the following substances, or combinations; from which I rejected the nitrous and marine acids, for obvious reasons, particularly on account of their volatility. I tried the fixed air procured from the vitriolic acid added to chalk, and even from the same acid and mild *volatile* alkali; that slowly obtained from fixed alkali and cream of tartar; and even that which rises from wort in the act of fermentation. The same signs of acidity were exhibited by the fixed air obtained from all of them.

In all these processes, however, an acid, mineral or vegetable, might be suspected to have been concerned as an ingredient in the process, in the production of the effects above ascribed to the fixed air. In the following experiment, therefore, I used fixed air expelled from a body, without the intervention of an acid, and merely by the force of fire.

EXPERIMENT VI.

A phial, to the mouth of which a glass tube had been joined, by means of the blow pipe, was filled with Magnesia, the perfect purity of which I had previously ascertained. Having placed it in sand contained in a crucible, which was set upon the fire, the air proceeding from it was successively received into small phials filled with infusion of Litmus. The first ounce, which came over even before the Magnesia could be thoroughly heated, though necessarily mixed with common air, tinged the infusion of a red colour. The subsequent produce (which came over to the amount of eight ounces, when the apparatus was accidentally broken) continued to exhibit the same signs of acidity, as were given by the fixed air, expelled from alkaline salts and earths, by means of acids. It possessed likewise the other properties of the last mentioned fixed air, which will be related in the next, and some of the following experiments.

Finding the acid in fixed air not strong enough, or sufficiently concentrated, to act sensibly on the generality of the blue vegetable juices; I conceived, that by the following method the greater part of them might nevertheless be made to bear testimony to its acid quality.

EXPERIMENT VII.

Having diluted some syrup of violets with water, and prepared different infusions of such of the blue vegetable flowers as were not changed red by fixed air; there were added to each of them a few drops of a solution of fixed alkali, sufficient to turn them to a
green

green colour. A few drops likewise of the same solution were added to an infusion of Litmus. On impregnating these different liquors with fixed air, (from spirit of vitriol and chalk) the infusion of Litmus was changed red as before; and the green colour given to the other blue infusions, by the alkaline salt, was destroyed by the fixed air:—an evident proof, that the *alkali*, by which the green colour had been produced, had been *neutralised* by an *acid*.

The same effects were produced by impregnating the infusions with the fixed air procured, in the preceding experiment, from Magnesia, by simple calcination;—as likewise from chalk calcined in a tobacco-pipe, and afterwards in a gun-barrel: tho' the greater part of the produce, in this last process, was, as you have already noticed, insoluble in water, and inflammable.

All these experiments, and others of a similar nature, proved only that an acid existed *in* fixed air. This last however induced me to extend my views, and suggested a series of experiments, which led me to the conclusion announced in the beginning of this letter;—that fixed air, when pure, and from whatever substance obtained, is only a peculiar acid, in a state of vapour; which particular modification it assumes on its expulsion from various bodies, by the power of a superior acid taking its place; or by that of fire. These experiments, which are as simple, as they appear to me to be decisive, shall be the subject of another letter.

WILLIAM BEWLY.

Great Massingham, Norfolk,
Sept. 23, 1775.

LETTER

LETTER II.

Great Massingham, Sept. 27, 1775.

I have hitherto attempted merely to ascertain the existence of *an* acid in fixed air. The avowed purpose of the present letter is no less than that of introducing a new subject into the tribe of acids; and of shewing that the aforesaid *acid* is, in fact, the very substance denominated fixed *air*. The following experiments will at least, I flatter myself, decisively prove that it is essential to the constitence of that fluid; and that it cannot be deprived of it, without ceasing to be fixed air.

Apprehending that, if the acid detected in fixed air were only a foreign or contingent principle, casually floating in this fluid, it might be deprived of this adventitious substance, by means of an alkaline salt, and yet still retain its other distinguishing characteristics, of elasticity, absorption in water, &c. I pursued the hint suggested to me by the event of the last experiment, by trying whether I could not divest it of this supposed adventitious acid, and thereby procure and examine it in a state of purity. For this purpose I first made the following experiment;

EXPERIMENT VIII.

Filling a two ounce phial with a strong solution of *mild* fixed alkali, and putting into a cup of a small diameter a very little quantity of the same solution, barely sufficient to allow me to immerge the neck
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of the phial into it, without suffering the common air to enter; I found that, on throwing about an ounce of fixed air into it repeatedly, and alternately taking the phial out of the cup, and agitating its contents, the fixed air *totally* * disappeared each time; and, upon the whole, in such quantities, as could not easily be accounted for, on any other hypothesis, than that it was merely the vapour, or the elastic fumes, of an *acid* spirit, condensed, and combined with an *alkali*. Several ounce measures of fixed air were thus made to disappear successively; and I, at length, discontinued the process, through mere lassitude.

If the alkali had only laid hold of an extraneous acid floating in fixed air, it might have been expected, that the aerial substance, or vehicle, which contained it, might have remained, with only some slight diminution of its bulk: but on every fresh introduction of fixed air, nearly the *whole* of it vanished; and the *alkali*, which was *mild*, evidently appeared to act, not as an absorbent of a supposed aerial substance, but as an *Antacid*.

I next made the following experiment, with water, in which I dissolved a small and known quantity of mild alkaline salt; in order to determine how much of the alkali a given quantity of fixed air was capable of neutralising.

* When I use this expression, or others of a similar import, here and elsewhere, I scarce think it necessary to observe, that a very small *residuum* was left after each trial; the space occupied by which I usually filled up, for the sake of expedition, from the liquor in the cup. I never collected these *residua*; which I consider as *impurities*, consisting, in part at least, of common air; from which, and inflammable air, it is scarce possible to procure fixed air perfectly free. Whatever they may be, they certainly are not the substance we usually design by the name of fixed air.

EXPERIMENT IX.

I impregnated five ounces of Well-water with fixed air, till it would receive no more. I could not make it absorb more than about four ounces. I then added to the water 20 grains of salt of tartar, previously dissolved in a small quantity of water; and immersing the mouth of the phial into a small cup, containing water, I threw up into it about half its bulk of fixed air. On agitating the liquor, and again immersing the mouth of the phial, and then slowly withdrawing my finger, the liquor in the cup, though the greatest part of it had been before *saturated* with fixed air, rushed up into the phial, with nearly as much violence as if a *vacuum* had been formed in the upper part of it.—The effect naturally reminded me of the condensation of steam or vapour in the fire engine; and is scarcely to be accounted for, without considering it as proceeding from a similar cause, or conceiving the included fixed air, as a greatly expanded and elastic vapour of an *acid* spirit, suddenly *condensed*, and immensely reduced in its dimensions, *que acid*, by the action of the *alkali*.—Fresh portions of fixed air, introduced into the alkaline solution, successively disappeared; and, upon the whole, in consequence of the addition of only these twenty grains of alkaline salt, the water received or condensed about seven or eight additional ounces of fixed air.

EXPERIMENT X.

That I might see the progress of the neutralisation, I diversified the preceding experiment, by colouring

water with Litmus, syrup of violets, and other blue infusions ; and by the change of colour induced, was enabled to see the action of the *mephitic acid* on the different alcalified liquors ; and in the infusion of Litmus particularly, could perceive the final predominance of the acid, as in the 7th experiment, by means of the red colour given by it to the liquor.

Should the foregoing evidence for the existence of the *mephitic acid*, founded on the visible changes of colour produced by it, be questioned ; it is corroborated, and indeed rendered unquestionable, by the testimony of another sense, in the following experiment.

EXPERIMENT XI.

The last experiment was repeated, with a larger proportion of alkaline salt ; each ounce of water now containing six grains of alkali. The solution had in a high degree the well-known acrid, urinous, and abominable taste of the alkaline salt. Tasting it, at different times during the course of the impregnation, the acrid and lixivial flavour was found to be gradually diminished, in proportion as the fixed air was combined with it. Towards the end, the alkaline and urinous flavour was completely destroyed, by the action of the mephitic acid ; and when the alkali was perfectly neutralised, the solution, which was coloured with Litmus, on being well agitated with fresh portions of fixed air, received still more of that fluid ; at the same time it became red, and its taste was now simply, and not disagreeably, saline, and even *subacid*.

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From these and some other experiments, I estimated, that an ounce of fixed air, or *acid mephitic vapour*, will neutralise between three and four grains of *mild* fixed alkali; or perhaps somewhat more. It was not indeed easy, by this method, to ascertain the exact quantity. Part of the mephitic acid was doubtless neutralised, even in its passage, in small bubbles, (as was the case in my experiments) through the alcalised liquor. On the other hand, it is difficult to know whether, and how far, this loss by *condensation*, was counterbalanced, or more than counterbalanced, by the *dissipation* at the surface of the liquor in the basin.

EXPERIMENT XII.

Effects similar to those related in the preceding experiment were produced, on adding the *volatile* alkali to water, and likewise the *fossil* alkali; but, as might be expected, in a less degree. It is supposed, that the latter owes the principal properties which distinguish it from the fixed vegetable alkali, to its containing a larger proportion of fixed air. As it is likewise frequently impure, if it should contain any of the marine acid capable of being disengaged from it; that acid, as superior to the mephitic, must contribute to prevent so large a portion of the latter from entering into the alkaline solution, as would be received when the pure vegetable alkali is employed.

EXPERIMENT XIII.

Having thus obtained a perfectly new *neutral salt*, (though in a state of solution) I was desirous of ascertaining

certaining some of its chemical qualities ; and particularly of trying whether fixed air, after having been neutralised by an alcali, might not be expelled from it by means of fire, and come over possessed of its acid quality.

I took therefore some of the produce of the 11th Experiment, and first neutralised the superabundant mephitic acid, by dropping in *Lixivium tartari* till the solution lost its red colour, and became blue. With this liquor I nearly filled a phial, to which a bent tube was accurately adapted, and well secured with very stiff cement. Putting it into a pan of water, placed on burning coals, I set a phial, filled with infusion of Litmus, over the extremity of the tube, which was immersed in a basin of water. I was surprised to find that no sensible part of the large quantity of mephitic air, or acid vapour, contained in the solution in a condensed state, was expelled from the alkali, though the water in the pan was made to boil violently : and yet innumerable small bubbles, probably the mere vapour of the heated liquor, were perceived to ascend from the bottom of the phial. A very small portion of air indeed came over, at the beginning of the process, into the inverted phial : but no part of it was absorbed by the infusion ; nor could any change of colour be produced in the latter by agitation ; neither did the saline solution, though so long subjected nearly to a boiling heat, exhibit, when cold, any taste of the alcali contained in it. To discover whether the apparatus might not have deceived me, I repeated the experiment in the same bottle with pure water, saturated solely with fixed air ; the greatest part of which came freely over, and ascended into the inverted phial.

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EXPERIMENT XIV.

Finding the mephitic acid thus resist nearly a boiling heat, when combined with the fixed alkali, but shut out from all communication with the common air; I was desirous of trying whether the neutral salt, formed of these two substances, might not possibly be procured in a concrete or crystalline form. But, on exposing different coloured solutions, the product of the 11th Experiment, in broad plates, to the common air, in a warm room; the early change of their colour soon convinced me—(though the great quantity of the mephitic acid, which alkaline salts contain in their common state, strongly adheres to them even in a considerable heat—) that the acid, superadded to that before combined with them, has a much greater affinity to atmospherical air, than to fixed alkali.—In a few hours, the flight of the mephitic acid was sensible to the taste; the infusions becoming gradually more sensibly alkaline. Having used only small quantities, I cannot speak precisely as to the particular nature of the salt left after the evaporation of the greater part of the water. In some of the plates, very small crystals were formed; but the greatest part of the solution continued deliquescent.—On the whole, the remaining salt did not appear to be *fossil alkali*.

EXPERIMENT XV.

Finding that fixed air acted as an acid, in perfectly neutralising alkaline salts, I was naturally led, from analogy, to expect that it might likewise dissolve calcareous earths. On adding the finest powder of common chalk to water, in a sufficient quantity to
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render it milky and opaque, I found, that on repeatedly and forcibly agitating the liquor with fresh portions of fixed air, its milkiness and opacity gradually disappeared. The whole of the earth was at length perfectly dissolved, and the water became transparent. —Pure magnesia was dissolved in the same manner.

When I tried this experiment, I did not recollect one made by the Hon. Mr. Cavendish, in his observations on what he calls the *unneutralised* earth in Rathbone-place water, and other waters *; by which he means an earth not dissolved or saturated by any of the known mineral acids, but suspended in water by an additional proportion of fixed air. The present experiment shews that it is neutralised, or dissolved, at least, by the mephitic acid.

It is remarkable, as he observes, that *pure*, or calcined calcareous earth, which is soluble in water, should, on being impregnated with fixed air, become totally insoluble in that fluid; and that by adding a still further portion of fixed air, it should be again rendered capable of being suspended in water. Considering fixed air as an *acid*, the singularity in a great measure disappears. More than one instance in chemistry occurs to me that resemble the foregoing. — Thus *calomel*, or mercury combined with the marine acid, is almost totally insoluble in water (one grain requiring near 2000 times its weight of boiling water to dissolve it); but *mercury sublimate*, or mercury combined with a still larger portion of the same acid, is very readily soluble in the same liquid †.

Since I wrote what goes before, the idea of an experiment occurred to me, which I immediately

* Phil. Transf. Vol. 57. p. 104.

† See Baume's *Chymie. Experimentale*, &c. Tom. 2. p. 428, &c.

executed ; and the result of which, though it comes in here rather out of its place, is too material and decisive to be omitted.

EXPERIMENT XV.

Recollecting that you had obtained fixed air, by means of fire, from volatile alkaline salts, I dissolved some of the volatile salt of *Sal ammoniac* in water, with which I nearly filled a phial fitted up with a bent tube, which I set on the fire in a pan of water. The fixed air which was expelled from this *alkaline* salt, *without employing any other medium than heat*, exhibited the very same *phenomena* with that procured by the intervention of foreign acids. Although much volatile alkali must have come over with it, and neutralised a considerable part of it ; yet the *mephitic acid* was so much more abundant, as not only to neutralise the alkaline vapours that rose along with it, but to be predominant in the coloured infusion into which it was received. This liquor was so far *acidulated* with it, as to become of a bright red ; and it required a sensible quantity of fixed alkali to restore its blue colour, and neutralise it.

EXPERIMENT XVI.

In consequence of the result of this experiment, (though I had before found [EXPERIMENT XII.] that fixed air could not be recovered, by means of heat, from a combination of it with *fixed* alkali) I neutralised several ounces of it, condensed in water, with *volatile* alkaline salt, and then added more of the same alkali ; till the liquor was very sensibly alkaline.

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Treating this solution in the same manner as in the 12th and the preceding experiment, I found, on the very first application of the heat of boiling water, that the fixed air left the volatile alkali, with which it had been intimately combined, and indeed *super-saturated*; and ascended, or was *distilled* over into the inverted phial, in copious showers; perfectly unchanged, from its union with the alkali, and possessed of its acid, and all its other, qualities.

EXPERIMENT XVII.

Under this title, I shall only recapitulate, as the general result of all the experiments I have made, with a view to analyse fixed air, and particularly to detach its acid from it, by means of alkalis;—that this acid is a principle *essential* to the constitution of this fluid; if indeed it does not constitute the whole of it. If a small quantity of alkali be employed, the remaining fixed air, which has been agitated with it, retains as much of its acid quality, as if it had never been subjected to the action of the alkali. On the other hand, if a sufficient quantity of the latter has been agitated with it, in order to neutralise the whole of the mephitic *acid*, the factitious *air* disappears. In short, fixed air, and its acid, if they be not one and the same substance, appear, from all my researches into the nature of this fluid, to be, at least, inseparable companions: they come and go together; so that, when the *acid* is destroyed, or loses its distinguishing characteristics, the *air*, at the same time, vanishes from our notice.

Some of these last-mentioned experiments have been so lately made; and the impression of your

second volume is, as you inform me, in such forwardness, that I have not time even to hint at the results of the numerous collateral objects of inquiry, which the consideration of this interesting and fruitful subject has suggested to me; and which I or others may hereafter prosecute. I am pretty confident that I have not been materially deceived in the experiments above related; or been tempted, by a predilection for a preconceived hypothesis, to draw conclusions not fully warranted by the premises. On that supposition I shall terminate this long letter, or rather formal essay, which I have not time, however, to shorten, with a few miscellaneous reflections, in the order in which they occur to me.

I. The ultimate design of all our experimental researches into the properties of natural bodies, is, or ought to be, public utility. On this account, I mention in the first place a practical, useful, and perhaps important application of the results of the eighth, ninth, and tenth experiments. A medical use may be made of the processes there described, in which a *new neutral salt* is produced, by combining the mephitic acid with alkaline salts; in putrid diseases particularly, and in all those cases where we would wish to introduce a larger quantity of fixed air into the system, than can be condensed by, or combined with, simple water. By previously dissolving in this fluid certain quantities of fixed alkaline salt, it may be made to receive twice or thrice its bulk, or a still larger proportion, of fixed air. I have not made any experiments purposely to ascertain how far the solubility of the *neutral mephitic salt* in water extends: but from the 7th experiment it should seem to be possessed of this quality in an unlimited, or at least in a very extensive degree. Neither have I yet
had

had opportunities of experiencing the qualities of very strong solutions of this new saline neutral compound. It is probable that they will be, at least in part, decomposed in the stomach, or *prima viæ*. I, once only, drank eight ounces of alcalised water, which had been neutralised by about a pint and a half of fixed air; and was very sensible of its effects, particularly in my head, for some time afterwards. It appeared likewise to act pretty strongly as a diuretic.

From the very short experience I have yet had of Dr. Nooth's apparatus (described in the last volume of the Philosophical Transactions) it seems to me well adapted to the preparation of this compound, or saline Pymont water. All the junctures, however, ought to be perfectly air-tight; as it is of advantage that the alcalised water should stand a few days exposed to the action of fresh portions of fixed air; that it may be perfectly neutralised, and even receive an excess of acid.

2. When a moderate quantity of alkaline salt has been dissolved in the water, as, for instance, only three or four grains in each ounce, the artificial Pymont water, into which I have converted this weaker alkaline solution, is of course more sapid, and appears to me more pleasant, than even that which has been made with simple water. It has the same acidulous taste, when the process has been properly conducted; and if it should be desired to have it still more pungent and acidulous, chemistry will furnish us with various expedients for disengaging a part of the mephitic acid, at the time the water is drank. This may be effected either by different saline compounds, or by naked acids, or acescent liquors. For the mephitic acid is let loose from its alkaline basis by all the

acids which I have yet had leisure to try ; and even by the slight and latent acid contained in wine, or other vinous liquors.

3. These experiments lead us to consider the common or *mild* alkaline salts, as they are called, in a new point of view. They shew, that the only true and simple alkaline salt is the *caustic* alkali, either fixed or volatile ; which has been reduced to a *pure* alkaline state by the abstraction of the mephitic acid combined with it, through the superior affinity or attraction of *pure* or *simple*, that is, *calcined* calcareous earth. All the others are only *subalkaline* salts, more or less combined with, and in part neutralised by, the mephitic acid ; and which are capable, as has been shewn, of being completely neutralised by it.

4. When we expel, and collect, the mephitic acid from an alkali, by means of the vitriolic, or any other acid, the process is perfectly similar to those by which we expel and distill other acids from their alkaline, earthy, or metallic bases, by means of a superior acid. The only material difference is, that the vapours of those acids, though equally elastic, are, in general, readily condensable, and come over reduced into a liquid and palpable state : whereas the vapour of the mephitic acid more obstinately retains its elasticity ; which it preserves till a body is presented to it, to which it has an affinity, and with which it then readily unites. Your *acid air*, that is, the marine acid in a state of vapour, approaches nearest to it in this respect. Like mephitic air, it preserves its elastic or aerial qualities, when confined by glass and mercury ; and only differs from it in being much more readily and copiously condensed, when water is presented to it. In that case, however, the phlegm which
condenses

condenses *acid air* becomes a strong spirit of sea salt; whereas that which condenses *fixed air*, does it so sparingly, as to constitute only a Pyrmont water, or a dilute solution of mephitic acid.

5. It is a matter which may be worthy of future investigation, to inquire whether pure *fixed air* be a simple or compound substance; and whether the *mephitic acid* may not be procured, *per se*, or in a liquid, visible, or concentrated state; by the addition of a few *drops* of which, water or other liquors may be impregnated with it to any degree. From the experiments related in a letter of mine inserted in your former volume, [page 317, 1st edit.] as well as from some of your own observations, it appeared that nitrous air was the *vapour* of the nitrous acid, probably combined with phlogiston, or some other substance*; to which it owed its elasticity, or aerial form, and from which it was separable by the admixture of atmospherical air. If fixed air be simply the vapour of the mephitic acid, the task of thus condensing or concentrating it becomes desperate. But it may possibly be united with some *volatilising principle*, to which it may owe its elasticity, and its being so sparingly soluble in water. Pursuing this idea, I foresee many resources which chemistry affords us, for accomplishing this purpose. As I have not, however,

* The union of this principle with the nitrous acid in nitrous air, is so strict, that the latter may be long and forcibly agitated in a phial, not only with water, but even with alkaline solutions, or lime-water, without being decomposed. But on holding the phial in an inverted situation, so as to suffer bubbles of atmospherical air successively to enter, the decomposition each time visibly takes place; and the redness and effervescence appear on every fresh admission, till the whole of the nitrous acid, hereby dislodged from the other principle, has been combined with the included alkali or earth.

had time to realise any of them, I shall not enlarge this Essay with any of my various speculations on this subject.

I am, &c.

WM. BEWLY.

See Mr. Bewly's *third Letter*, No. VI.

N U M B E R II.

A Letter from Dr. PERCIVAL, F.R.S. and S.A. to the Rev. Dr. PRIESTLEY, on the Solution of Stones of the Urinary and of the Gall Bladder, by impregnating Water with FIXED AIR.

Manchester, June 1, 1775.

Dear Sir,

I flatter myself that FIXED AIR is now become an object of the attention of physicians; as it has been fully shewn that it is capable of being applied to many important medicinal purposes. In *pulmonic disorders*, the *gangrenous sore throat*, and in *malignant fevers*, the happiest effects have been experienced from the use of it; and I know not a more powerful remedy for *foul ulcers*, as it mitigates pain, promotes a good digestion, and corrects the putrid disposition of the fluids. I have related several cases, in the Appendix to your treatise on air, which evince the truth of these observations; and since the publication of that work, a variety of similar facts have occurred to my learned friend, Dr. Dobson, in his hospital-practice at Liverpool.

But I have a farther and very interesting discovery, concerning the medicinal properties of this species of factitious air, to communicate to you. About the end of last year I was informed that Dr. Saunders,
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a physician in London, eminent for his knowledge of chemistry, had employed it as a solvent of the human CALCULUS.

I was ignorant of the manner in which his trials were conducted, and of the success which had attended them: but my curiosity was excited; the acquisition of such a remedy was flattering to my hopes; and I engaged in the pursuit of it almost with as much ardour, as if it had been the philosopher's stone. I recollected that Dr. Black and Mr. Cavendish have proved the solubility of various earthy bodies in water, either by abstracting from, or superadding to the fixed air which they contain: and as the human calculus is dissolved in the former way by lime-water and the caustic alkali; it appeared highly probable, that the like effect would be produced on the same substance by the latter mode of operation. Analogy seemed favourable to the hypothesis; and experiment has confirmed it. I have found by repeated trials that *calculi*, extracted from different subjects, and varying in size, figure, and texture, are soluble in water impregnated with fixed air; that this menstruum is more powerful in its operation even than lime-water; and that though it is inferior in efficacy to the vitriolic acid, and the caustic alkali, yet it is more universal in its action than either of them. For it is well known (see Dr. Dawson's Experiments, Medical Transactions, vol. 2. p. 105) that some stones, which are dissolved by the caustic alkali, are unchanged by the vitriolic acid, and *vice versa*; whereas the mephitic water, as far as my observations have reached, acts upon every *calculus* which is suspended in it. And I have tried it with those which have suffered no diminution of weight from
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from the *menstrua* above-mentioned. I do not trouble you with a detail of my experiments, because they would exceed the bounds of a letter, and I shall probably publish them, with such remarks as they may suggest, on some future occasion.

But I cannot restrain myself from expressing the heart-felt satisfaction which I enjoy in the discovery of a new lithontriptic medicine, that is at once grateful to the palate, strengthening to the stomach, and salutary to the whole system. Lime-water often nauseates the patient, destroys his appetite, and creates the heart-burn: and the soap-ley is so caustic and acrimonious, that it can be taken only in the smallest quantity; frequently produces bloody urine; and aggravates the tortures which it is intended to relieve. Both these remedies also require a very strict regimen of diet, and their qualities are liable to be changed either by acidities, or the fermentation of our food in the first passages. But the mephitic water may be drunk in the largest quantity without satiety or inconvenience: it requires no restrictions in diet, and its medicinal virtues will be undiminished in the stomach or bowels. Perhaps it may be questioned whether fixed air can be conveyed by the ordinary course of circulation to the kidneys and bladder: In an elastic state it certainly cannot; but dissolved in water, it may pass through the vascular system, without creating the least disturbance or disorder; and by its diuretic quality, will be powerfully determined to the urinary organs. So strong is the relation that subsists between mephitic air and water, that they remain firmly combined, although exposed to considerable variations of heat and cold. You found that it required half an hour, even when the boiling heat was employed,

employed, to expel compleatly the fixed air from a phial of impregnated water ; and I have observed that it has retained its peculiar flavour several days, when left in a bason, with a large surface open to the external air.

But to obtain more satisfactory evidence upon this subject, I filled a bottle with mephitic water, and placed it in a heat of about 98 degrees of Fahrenheit's thermometer. A bent glass tube, a quarter of an inch in diameter, properly luted at each end, formed a communication between this bottle and one of lime-water, to the bottom of which it extended. An intestine motion soon ensued ; air-bubbles were slowly conveyed into the lime-water ; and a white precipitation was gradually formed.

In an hour and a half the lime-water was become turbid ; but was quickly rendered quite milky by blowing air into it from the lungs. The mephitic water still retained its brisk acidulous taste ; and when a greater degree of heat (108°) was applied to the bottle which contained it, a brisk intestine motion was renewed.

As the vapour of chalk, and oil of vitriol, has been found so efficacious in correcting the *sanies*, and abating the pain of foul ulcers, when externally applied, we may reasonably presume that the internal use of the same remedy will prove beneficial in similar affections of the urinary passages. Such complaints frequently occur in practice, and may arise either from *calculi* in the kidneys and bladder ; from the recession of scorbutic eruptions, which appeared on the surface of the body ; from the venereal disease ; from strains ; from contusions ; or various other causes. And water impregnated with fixed air, seems well adapted, by its diuretic, healing, and antiseptic powers,

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to wash off, and sweeten the acrid matter, to abate the defluxion on the mucous membrane, to contract the flabby edges of the ulcers, and to dispose them to a speedy granulation. If the pain, inflammation, and absorption of *pus* have excited a hectic fever, the patient may drink plentifully of Seltzer water, which is of a cooling quality, although it abounds with mephitic air: or a small quantity of Rochelle salt may be added to the mineral water artificially prepared. Thus will the increased action of the heart and arteries, which may arise from the stimulus of the fixed air, be entirely obviated, without the least diminution of its medicinal powers. And whilst the sanction of experience is wanting, reason will justify the trial of a remedy, which is, at once, safe, pleasant, and efficacious.

In ulcers of the kidneys and bladder, the urine is commonly high coloured, pungent, and of an offensive smell. To ascertain whether fixed air would correct these qualities, I attempted the following disagreeable experiment.

Repeated streams of fixed air were conveyed into three pints of urine, which had been kept till it was become very putrid, and which emitted a strong volatile odour. I examined the smell of it from time to time, whilst this process was carrying on, and compared it with a portion of the same urine which was reserved as a standard. The pungency of it gradually diminished; it acquired a brighter colour, and was less turbid; but its putrid odour seemed to be increased. These observations were made in the evening, and early the next morning I awoke with a violent head-ach, which was attended with a vomiting and a *diarrhœa*. Alarmed at these effects,
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which I attributed to the putrid vapours of the urine, I dropped the prosecution of the experiment; but the succeeding day, Mr. Thomas Smith, a young gentleman, who will one day be an ornament to the profession of physic, undertook the examination which I had begun: and after attentively comparing together the standard and the urine impregnated with fixed air, he found the latter more offensively putrid than the former, but without any degree of pungency or volatility. As this experiment was not completed, I am uncertain whether the urine was sweetened by the mephitic air. But it is evident that the volatile alkali, generated by putrefaction, was either neutralised, dissipated, or prevented from ascending by the atmosphere of fixed air, which filled the upper part of the vessel. Perhaps this atmosphere might be the *menstruum* of the putrid *effluvia*, emitted by the urine, which being then accumulated, would appear to have its fœtor increased. In another work, I have related an experiment of Mr. Henry's somewhat similar to this, and which suggested to him the like explanation. A piece of putrid flesh was suspended twelve hours in a three-pint bottle closely corked, and filled with fixed air, which had been separated from chalk by the vitriolic acid. The beef was considerably sweetened, but the air in the bottle was rendered intolerably offensive.

The waters of Bath, in Somersetshire, have been long and justly celebrated for their efficacy in the jaundice, and other hepatic disorders. They abound with fixed air; and it may be of importance to ascertain whether they derive from this active principle, the power of dissolving the concretions of the bile,
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and of removing the obstructions in the liver. I was induced therefore to try the solubility of gall stones in mephitic water. But I have yet only a solitary experiment on the subject to offer to you. A gall stone, that had been extracted from a tumour in the region of the liver, was divided into two parts. One of these, which weighed fifty-one grains and a half, was immersed four days in rain water, strongly impregnated with fixed air. The other weighed twenty grains and a quarter, and was macerated in simple rain-water during the same space of time. The first fragment, when carefully dried, was become heavier by one grain, having gained so much from the fixed air. In texture and appearance it remained unchanged. The second fragment had lost one eighth of a grain.

I mean not to draw any decisive inference from a single experiment. But it is probable that the Bath waters resolve concretions of the bile, not so much by a chemical operation, as by accelerating the secretions of the liver, stimulating the organs of digestion, and invigorating the whole animal system. Nature indeed observes a peculiar oeconomy in the circulation of the blood through the liver; and as the bile is one of her most elaborate fluids, it must be difficult to introduce a foreign and unassimilated substance into it. From analogy, however, we may conclude, that this is not impracticable. The milk and the saliva are frequently impregnated with adventitious matters; and these animal liquors, like the bile, are secreted by organs of a particular structure, and for determinate and important purposes. A remedy which would pass unchanged into the system of the liver, and medicate

dicare the bile, so as to render it unapt to coagulate, or enable it to resolve the concretions already formed, would be a most valuable acquisition *; and the obstacles to the attainment of it should rather be regarded as incitements to our industry, than apologies for supineness and despair. Such, it must be acknowledged, they have proved; as appears from the variety of dissolvents which have been proposed and tried. Acids, alkalies, soap, ardent and dulcified spirits, with fresh vegetable juices, have been recommended. Valisnerius found that a composition of alcohol and oil of turpentine destroyed the texture and cohesion of gall stones, more perfectly than any other *menstruum* †; and Mr. William White of York has fully confirmed this observation, by a number of judicious experiments which he has communicated to me. Some time ago I thought favourably of this remedy, and endeavoured to promote the trial of it ‡; but farther reflection has convinced me, that the continued use of it is more likely to prove injurious than beneficial. Spirituous liquors, of all sorts, have a peculiarly unfavourable operation on the liver; and it would be absurd to seek a *specific medicine* for the diseases of the bile, in what experience has fatally shewn to be a *specific poison* to the organ which secretes it. Perhaps fixed air, under some form or other, may hereafter be found to be the *desideratum*, which we have been so long pursuing. At least, we may be allowed to attribute some share of the virtues which the Bath waters possess, to this ingredient in their composition; and when they cannot be employed, to

* Vide Medical Transactions, vol. 2. p. 165.

† Opere, tom. 3. p. 6.

‡ Essays Medical and Experimental, vol. 2. p. 232.

recommend the mephitic water, as an innocent and efficacious substitute.

I am, with sincere respect and esteem,

Dear Sir,

Your most faithful and affectionate friend,

THO. PERCIVAL.

P. S. Since this letter was written, the young gentleman, whose name I have before mentioned with respect, has at my desire taken large quantities of mephitic water daily, during the space of a fortnight. His urine became impregnated with fixed air, precipitated lime-water, and proved a powerful dissolvent of the *calculi*, which were immersed in it.

Dr. Saunders, to whom I have communicated my observations on the solution of human *calculi* by mephitic water, has lately favoured me with a general account of his discoveries on this subject. There is a perfect agreement in the result of our experiments, and we propose to publish them together.

NUMBER III.

A Letter from Dr. DOBSON of Liverpool to Dr. PRIESTLEY; with Cases of the Efficacy of FIXED AIR in putrid Disorders.

Liverpool, March 29, 1775.

Dear Sir,

My friend Dr. Percival has lately informed me, that you are about to publish a supplement to your valuable work on fixed air.

The gentlemen of the faculty have not given that attention to the medical uses of fixed air, which I think it merits: and a late medical writer, (vide Dr. Lettsom's Medical Memoirs of the Gener. Dispens. p. 334.) doubts whether fixed air has any real efficacy even in diseases of a putrid class. I have transcribed therefore four cases from a number of others, which are much at your service. If they are too late, or do not coincide with the intention of your present publication, please to return them, that they may be joined with some other cases and practical observations, which I shall give to the public as soon as I have time to transcribe them.

That Dr. Priestley may long enjoy health, and the world reap the fruits of his philosophical labours, are the sincere wishes of his

Respectful and obedient servant,

MATT. DOBSON.

*Of the Efficacy of FIXED AIR in Fevers of the
putrid Class.*

Putrid fevers rarely acquire any great degree of malignancy in Liverpool, or its neighbourhood ; and when they do appear, it is generally among the lower ranks of people. A fever of this kind crept into our public hospital in the spring of the year 1773, and a considerable number were infected.

The following histories are transcribed from the notes, which were taken during my attendance on the respective patients.

History I.

Mary Rainford, about 15 years of age, was admitted into the hospital on account of convulsions ; she was subject likewise every three or four weeks to vomit large quantities of blood, and was much enfeebled by these complaints at the time of her being seized with the fever.

She first complained of pain and weight in the head, pain in the limbs and back, and a great degree of languor and dejection ; she had frequent chills, alternating with flushes of heat, and got very little rest. The tartar emetic was twice given, and operated easily and powerfully by vomit ; a blister was applied between the shoulders ; and a dose of the following mixture was ordered to be taken every three hours.

R. S.

R. *Sp. Minderer.* ℥viij.

Sacch. Alb. ℥iij.

Sp. Lav. com. ℥ss. *M. Cap. Coch. ij. maj.*
tertiâ quaque hora.

She had for common drink lemonade, with sweet mountain, or barley-water well acidulated; the body was kept soluble either by clysters, or some gentle purgative, and the room was well aired by opening the door and windows. But notwithstanding the steady use of these means, the fever became more and more untoward, and was on the sixth day accompanied with such dangerous symptoms, as made it necessary to adopt some other method. The eyes were heavy, the conjunctiva red, large petechiæ spread over different parts of the body, the tongue was covered with a brown fur, and the teeth with a fur of a blackish colour; she was very feeble, got no sleep, and was frequently delirious, especially in the night. Hitherto the state of the pulse had been about 120, now it was 135, and very weak.

Fixed air was now directed in the following manner.

R. *Sal. Tartar.* ℥j.

Aq. Fontan. ℥℞.

Sacch. Alb. ℥j. *M. Sumend. cum Sacc. Limon.* ℥℞,
incipiente ebullitione; et omni hora repetend.

From the time she entered upon this plan, the symptoms were more favourable, she took no other medicine, and was out of danger in four days.

History II.

Alice Rigby was received into the hospital for a fore leg, and during her stay was attacked with the fever of the house. The progress and treatment of the disease for the first week, were nearly the same as in the preceding case. On the seventh day she was extremely weak, got no rest: there were large petechiæ on many parts of the body, the brain was much affected, pulse 125, and the tongue little differing from its natural appearance.

Fixed air was now administered in the same manner as to the former patient.—The petechiæ soon began to disappear, she got strength, the pulse became fuller and slower, and the fever was subdued in six days by the use of this medicine alone. The bark was at this time ordered, as an additional security against a relapse.

History III.

March 20. A consultation was desired for Ann Knowles, who had been in the hospital for a considerable time, and was much reduced by a long continued rheumatism at the time she was attacked with the fever. This was the seventh day of the disease; and though she had been very judiciously treated by the gentleman under whose care she had been at first admitted, the fever grew daily worse, and was now accompanied with many dangerous symptoms.

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I observed an extreme languor and dejection; the eyes heavy, the eye-lids half-closed, and the conjunctiva inflamed. There was a stupor, with a muttering kind of delirium, and a continual tossing and moaning. The pulse very weak and very frequent, more than 140 in a minute; the tongue moist and clear, and not altered from its natural appearance, except that it was of a deep red. The whole body was covered with small petechiæ; she had frequent stools, which were extremely offensive, and her little remains of strength were every hour still more and more exhausted.

It was agreed, that she should take the fixed air in the same way as I had ordered it for the two preceding patients.

March 21. The good effects of this medicine were evident, though the symptoms were still urgent and alarming: the stools less frequent, but offensive: the pulse 130, and not so languid: in other respects the patient was not much altered.

23. The petechiæ disappearing, the looseness diminished, and the stools much less offensive; pulse 110, sleeps and gets strength. The medicine was now to be given only every four hours.

24. Stronger and better, pulse 100, head much clearer, and the tongue has more of its natural red.

26. Pulse 85; and from this time the fever entirely left her. She took no other medicine, and had no relapse.

I have directed fixed air both in hospital and private practice for a variety of patients, in diseases accompanied with symptoms of putrefaction, and with success. It would be superfluous to enter into a further detail of particular histories. The following ac-

count, however, of the happy effects of fixed air in the second fever of the small pox, is so striking a proof of its efficacy in diseases of the putrid class, that I must transcribe it.

History IV.

Ann Forbes, servant to Mr. Hume of York-street, Liverpool, had the confluent small pox in August 1773. The weather was extremely hot, and the symptoms so very unfavourable, that there did not appear the most distant hope of her recovery. Particular care was taken to have a constant supply of fresh air, and the antiphlogistic treatment was strictly pursued during the inflammatory fever. The disease was now advancing into the putrid stage, and the second fever commenced with little or no appearance of suppuration.

Notwithstanding every precaution with respect to the free access of air, change of linnen, and every circumstance of cleanliness, the unlimited use of oranges, lemonade, and wine negus, this poor creature was the most miserable object I ever beheld. She became extremely offensive, and had the appearance of one continued mass of putrid ichor: the pulse small and rapid, and she had a constant restlessness with inexpressible anxiety.

A purgative was directed, and a glass of sweet mountain after every stool. The symptoms, however, became more and more alarming, the offensiveness was almost intolerable, and she was frequently sick, agitated, trembling, and like one about to expire. In this urgent situation, I determined to
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try the effects of fixed air, and it was given in the manner already mentioned.

The nurse and attendants soon observed an agreeable change. In 24 hours, the putrid stench was much diminished, the breath of the patient was not near so offensive, and the room was very tolerable compared with what it had hitherto been. She was cooler, had less anxiety, and the pulse became fuller and slower. In two days more, she was still much better : and by repeating the purgative, giving wine occasionally, and persevering in the use of the fixed air, her recovery was surprizingly speedy and perfect.

N U M B E R IV.

*Extract of a Letter from JOHN WARREN, M.D.
of Taunton, to Dr. PRIESTLEY, with a me-
dical Case, proving the Use of Clysters of FIXED
AIR in a putrid Disease.*

Taunton, Oct. 3, 1775.

Sir,

In compliance with your request, I have done myself the honour of sending you an account of a medical case, in which the application of *fixed air* proved so remarkably successful, as to leave no room to doubt of the great advantages the medical world might derive from its use, in putrid diseases of the worst and most formidable species.

I have for many years past been strongly prepos-
sessed in favour of the utility of fixed air in certain

medical cases, from its peculiar virtue verified by Dr. Macbride's experiments, in sweetening putrid flesh, and restoring to it that texture, which it must of course have lost by undergoing such a change; nor have I, in the practice of my profession, found myself disappointed of the advantages which I flattered myself from theory, might be derived from its use.

I have latterly employed it in almost every putrid case that offered; and though I am by no means so partial to its virtues, as to attribute the whole merit of a recovery to it alone, when probably other medicines, with which it was joined, were also entitled to their share, yet I am thoroughly convinced, that the success I have met with, in the treatment of putrid disorders, is, in a great measure, to be ascribed to the large quantities of this fluid which I have constantly enjoined my patients to make use of.

Various have been the modes which I have adopted for introducing it into the system—I order it frequently to be given by clyster, sometimes to be inhaled by the mouth (particularly in ulcerated fore-throats with the greatest advantage) and it is with the same view also of correcting putrefaction, that the common drinks, which I allow my patients, are almost all of them impregnated with this species of air, as Pyrmont water, brisk small beer, currant wine, and the like.

I would beg leave here to subjoin a practice I have for some time past found productive of the most beneficial consequences in the treatment of putrid diseases in children—It is commonly known, that physic of every kind is to them peculiarly obnoxious, and thousands annually fall a sacrifice to disorders from a mere antipathy to it. Therefore, whenever I perceive a child utterly averse to take medicine, if the
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Peruvian bark is proper, I give it by clyster, and order the milk (its best and most common vehicle when given in this manner) to be as thoroughly impregnated with fixed air as possible.

I have the honour to subscribe myself,

Sir,

Your very obedient servant,

JOHN WARREN.

The Medical Case.

Mr. C——, aged 23, a gentleman of great temperance, and of a good constitution, laboured under an irregular nervous fever for the space of ten weeks, attended for the most part with delirium, and almost constant watchfulness.

At the expiration of this period, symptoms of putridity began to make their appearance, seemingly however more owing to emaciation, and to the long continuance of his disease, than to any original contagion. The Peruvian bark had from time to time, particularly during the latter stages of his illness, been administered to him, which, on the appearance of putrefactive symptoms, was increased to the quantity of two scruples, given in the form of a bolus, with thirty drops of elixir of vitriol, every two hours. Every thing he drank was acidulated with the juice of lemons or oranges, and his common liquor was Port wine mixed with Pyrmont water. This course was persisted in for some days, the putrid complaints
however

however increased, and so great was the fœtor emitted with his breath, and arising from his body, that notwithstanding his being supplied with a constant succession of fresh air, and though large quantities of vinegar, lavender-water, and rue, with other odoriferous substances, were constantly made use of to purify his room, yet all were found perfectly ineffectual. His stools, which at this period consisted of little else than putrid blood, and which came from him in great abundance (in the whole to the amount of many pounds) were absolutely intolerable, nor was it without much difficulty that the nurses could be induced to remain any longer near him.

Universal languors, with almost total insensibility, now supervened: an earthy coldness diffused itself through every part of his body, nor were the hottest fomentations, though continued three hours together, capable of procuring him any degree of warmth.—Every breath he drew seemed to be his last.—In this deplorable situation he continued a whole day, his boluses were omitted through necessity, and with difficulty we could, from time to time, get him to swallow a few spoonfuls of some warm cordial medicine; which, however, by continually repeating, began at length, in some degree, a little to revive him.

I now ordered him clysters of *fixed air*, of which a large bladder full, containing near two quarts of air, was every three or four hours injected, and his bark boluses were again given to him, as often as his stomach would allow him to take them. In the space of eighteen hours, the cadaverous fœtor arising from him, began to abate; large vibices, or putrid blotches, were now, for the first time, discovered on almost every

every part of his body, his pulse however was better, and his warmth in some little degree returned; the boluses and clysters were ordered to be continued. In four or five days the noisome smell became imperceptible, the vibices gradually diminished, and his fever left him. He is now perfectly recovered, and a living miracle of what fixed air, under Divine Providence, is capable of effecting on the human oeconomy, in cases of the worst and most putrefactive nature.

JOHN WARREN.

N U M B E R V.

A Letter from Mr. MAGELLAN to Dr. PRIESTLEY, on the Subject of DEPHLOGISTICATED AIR.

Dear Sir,

Among the many important discoveries for which the philosophical world is indebted to you, chiefly in that new and extensive branch of natural knowledge respecting *different kinds of air*, a very striking one is the exhibiting, in the form of this fluid, many solid bodies, and most of the known acids. It is with the most pleasing astonishment that I have always beheld that experiment, by which any unprejudiced mind must be convinced, that atmospherical air, even the purest, and the fittest for animal respiration, is produced by heat from a mixture of nitrous acid with any dephlogisticated earth, as *red lead, chalk, &c.* For, after having extracted from this mixture all the air that fire can expel, which is a prodigious quantity, when you repeatedly add fresh nitrous-acid to
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the residuum, you get a fresh quantity of this purest air, till all the earthy substance disappears.

This, however, being a very tedious process, when carried on with the most scrupulous attention, it came to my mind, that it would be sufficient to examine at the end of the first process, whether the residuum from the above mixture contained any part of the nitrous acid which had been put to it. To clear up this doubt, I kept for that purpose, with your approbation, the very same residuum of that process of the kind which we made a few days ago, to shew this wonderful kind of air to his Highness Prince Orloff, and with it I made the following experiments.

I put into a large phial a weak blue tincture of archil, and after mixing it well, I poured two thirds of it into two smaller phials, in one of which I put a good quantity of the said residuum, and into the other as much of the dried mixture of red lead, with nitrous acid. The blue colour of this last phial disappeared in a few seconds, leaving the liquor almost limpid and transparent; but the other tincture, with the residuum out of which the air had been expelled, shewed no change of colour, when compared with the remaining quantity of the tincture left in the large vessel.

I repeated this morning the same experiment with the tincture of turnesole, and found the same effect, with this only difference, that the tincture turned reddish in the glass, which contained the dried mixture of red lead with nitrous acid, whilst the other kept its blue colour.

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This seems to evince, with the greatest certainty, that the nitrous acid in this experiment is entirely set free by the action of the fire, in the form of air, and being at the same time combined with some part of the earthy matter, becomes respirable air. It is remarkable that this air has no acid in it, as may be concluded from the effect of shaking it with the above tincture in a phial, which I did : for it does not change the blue colour ; whereas if the same operation is made with fixed air, it is changed almost instantaneously into a very decided reddish colour, as is well known.

Now since the air produced from the mixture of earth with nitrous acid, not only does not discover the least acidity, but proves to be the purest and the most wholesome for animal respiration, it plainly demonstrates that either air is not an *element*, or acid is not one, as some chuse to assert : since nitrous acid is reduced into air, together with the earth, in the above experiment, without leaving behind any acidity to impart it to the air which comes out from it. As to myself, I should rather think there is a transmutation of *elements* into one another, if such *name* may be used in this case ; for we see by the above experiment, that acid and earth are transmuted into air, and by the experiments of Mr. *Godfrey*, published in 1747, it seems that *water* is convertible into earth.

If you think the above may be any elucidation or confirmation of your experiments on this subject, you are at liberty to make what use of them you please.

Dear Sir, Yours, &c.

London, 20th Nov.

J. H. DE MAGELLAN.

1775.

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NUMBER VI.

Mr. BEWLY's *third Letter to Dr. PRIESTLEY*,
 containing *farther Experiments and Observa-*
tions on the mephitic Acid. See p. 337, &c.

Sir,

On perusing some of the first sheets of your new volume, which you have been so obliging as to transmit to me, together with a few pages of a treatise just published by Sig. Landriani of Milan *, relative to the subject of my former letters; I find that some of the foreign philosophers, who acknowledge the existence of an acid in fixed air, consider it only as an extrinsecal principle furnished by the particular acid that has been used in the process for procuring it. Sig. Landriani, in particular, who, from the specimen which you have sent me of his work, appears to be a very intelligent and accurate inquirer, there affirms, that the fixed air expelled from chalk by the *vitriolic* acid, and received into an inverted phial plunged in mercury, produces, on the admixture of alkaline air, crystals of *vitriolic* ammoniac; and that when the *nitrous* acid has been employed, a *nitrous* ammoniac is formed, which deflagrates without the addition of any phlogistic matter.

The same philosopher affirms (with a view to shew that the acid in fixed air is only a modification of the particular acid employed in generating it) that a given

* Intituled, *Ricerche Fisiche intorno alla salubrità dell' Aria.*

quantity of the infusion of turnesole, which will be changed red by a *certain quantity* of fixed air expelled from chalk by the strong vitriolic acid, will not have it's colour altered by an *equal quantity* of fixed air procured by a weak vegetable acid, as that of lemons. He further asserts, that fixed air betrays, even by it's smell, and by the flavour which it imparts to the water saturated with it, the particular acid to which it owes it's acidity. He particularises the fixed air expelled from chalk by the nitrous acid, and that expelled by the juice of lemons; observing, that the particular smells and flavours of these two acids may be distinctly perceived in the fixed air respectively procured by them, as well as in the water impregnated with them.

In these particulars I apprehend that Sig. Landriani has been deceived by appearances, and particularly by attributing to fixed air, in general, the attributes of fixed air accidentally *sophisticated* by foreign admixtures. This fluid undoubtedly, like all other fluids, is liable to receive taints or impregnations from any substances capable of being elevated into vapour, and of being dissolved or suspended in it. The hypothesis maintained by him and by other philosophers on the continent, with respect to the foreign or adventitious origin of the acid in fixed air, very naturally occurred to myself at the beginning of this inquiry: but the experiments and observations contained in my two former letters, as well as others which I suppressed, obliged me to renounce it, and to consider fixed air as an *original* acid, which does not owe it's acidity, much less it's existence, to any of the acids, or other *media*, which are employed in generating it. I could not indeed entertain any doubt
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of the truth of this last opinion, when I had procured fixed air (as is related in my first letter) exhibiting unequivocal marks of acidity, (that is, reddening the infusion of litmus, or neutralising alcalis) when expelled from chalk, the purest magnesia, and volatile alkaline salts, *by heat alone*. It may not be amiss however, before I proceed further on this subject, to take this opportunity of adding some of my former Observations relating to it, which I before omitted to mention; particularly those respecting the nature of the acid in the fixed air procured from chalk, by the *vitriolic acid*, and which Sig. Landriani calls *vitriolic fixed air*. That this acid is not vitriolic acid, under any of it's modifications known to us, appeared to me to be evident from the following considerations.

1. The acid in fixed air, thus obtained, dissolves a mild calcareous earth †, and on evaporating the water by means of heat, or adding an alcali, a mild calcareous earth is precipitated; whereas the vitriolic acid will scarce dissolve a sensible portion of the same earth, and the precipitate is a *selenite*.

The common or fixed, as well as the *volatile* or *sulphureous*, vitriolic acids, when neutralised with vegetable alcali, form neutral salts which continue neutral, though exposed to the air and to heat; the first constituting vitriolated tartar, and the second, the *sal sulphureus* of Stahl; which last, on exposure to the air, loses only the *phlogiston* to which it owed its volatility, and constantly retains its neutral quality: whereas the neutral *mephitic solution*, or the combination of vegetable alcali with the acid of (*vitriolic*) fixed

† See Letter 2d, Experiment XV.

air (in the 14th Experiment) parts with that acid in the common temperature of the atmosphere; and on the total evaporation of the water, the fixed alkali is left, very little changed by the experiment.

3. Solutions of vitriolic ammoniac will bear being evaporated over the fire to a pellicle; and, on cooling, perfectly neutral crystals are formed; but the ammoniacal solution formed by the union of fixed air (obtained as above) with volatile alkali, could not be made to furnish crystals; as the greatest part both of the acid and the alkali flies off, though exposed only to the common heat of the atmosphere.

4. The presence of the smallest portion of the vitriolic acid, combined with alkaline salts or earths, is easily detected by the precipitation of a *turpeth mineral*, on the addition of a saturated solution of mercury in the nitrous acid; whereas a strong *neutral mephitic solution*, treated in the same manner, furnishes only a white precipitate.

I shall now add some other observations of a similar kind; as it may be alledged, though no proofs have been offered for that purpose, that the vitriolic acid may possibly be volatilised, and acquire some new qualities, or may have its usual properties disguised, in consequence of combining it with calcareous earths or alkaline salts, in the common process for obtaining fixed air. I shall therefore proceed to consider the peculiar qualities ascribed by Sig. Landriani to the acid contained in the fixed air which has been procured from these substances by means of the *nitrous acid*.

I thought it very probable, when I entered on this investigation (as I have already hinted in one of my

former letters, that, considering the very volatile nature of the nitrous acid, some part of it might be elevated, during its effervescence with the chalk, either in the state of a simple vapour, or in the more compound form of *nitrous air*, so as to mix with and contaminate the fixed air obtained by its means. For similar reasons, respecting the marine acid, I avoided, in the preceding analysis, employing any fixed air, procured by means of either of these two acids. I made however the following experiments on the present occasion.

EXPERIMENT XVIII.

Having more than once prepared *artificial Pyrmont water* with fixed air, obtained by *spirit of nitre* and chalk, I could never distinguish it, either by its taste or smell, or *strength*, from the artificial Pyrmont water procured by means of the juice of lemons, or even the weak vegetable acid in cream of tartar, combined with salt of tartar. On neutralising the first mentioned waters with salt of tartar, and suffering a part of the phlegm to evaporate, I sometimes found that paper, dipped into the liquor, then dried, and applied to a hot cinder, exhibited, as indeed I expected, some very slight symptoms of the presence of the nitrous acid, by a faint deflagration. On a further evaporation, the liquor, which was originally neutral, or even subacid, had acquired a taste manifestly alkaline: a considerable part of the acid which had before neutralised it (that is, according to my theory, the *mephitic* acid) having flown off; while the nitrous or foreign vapours, which

which had *accidentally* been suspended in the fixed air, and which had been condensed by the water, combined with and fixed by the alkali, appeared to have produced these faint appearances of deflagration.

EXPERIMENT XIX.

Though I failed of procuring crystals from a neutral, but weak mephitic solution, when it was exposed to the atmosphere *; I succeeded, on using the strongest alkaline lixivium, and carrying on the process in the *medium* of fixed air.—A large quantity of fixed air, procured by the *nitrous* acid, having been thrown up into a small quantity of the strongest *lixivium tartari* confined by quicksilver, small crystals were almost instantly formed on the sides of the glass; and the liquor, after a repetition of the process, had condensed 12 or 14 times its bulk of fixed air: *but these crystals were not found to be nitre*; nor did they, or the liquor, exhibit any stronger marks of the presence of the nitrous acid, than those mentioned in the preceding experiment. On the contrary, the crystals, in particular, being carefully collected and thrown on a red-hot coal, did not detonate, but some of the particles bounced, or flew about; in consequence, doubtless, of the imprisoned fixed air being suddenly let loose by the great heat; and they were finally converted into fixed alkali. Volatile alkaline spirit, in the caustic state, treated in the same manner, presented nearly the same phenomena, *mutatis mutandis*. In

* See Experiment XIV.

short, the fixed air, in both cases, whenever it exhibited any appearances of the presence of the nitrous acid, seemed only to have been adulterated with the vapour or fumes of that acid, suspended in it, and condensed along with it.

The objection which, as I have above hinted, might be made with respect to the *vitriolic* acid, does not seem to be in any degree applicable to the present case. Sig. Landriani cannot allege, consistently with his own experiments, and his deductions from them, that the *nitrous* acid is so very materially changed in the act of effervescence with calcareous substances, as to have its nature so far altered, so that on being afterwards neutralised with fixed or volatile alcalies, it should be almost totally deprived of its *detonating* quality :—as the only proof which he offers of its presence in the fixed air expelled by it—(the *detonation* of his nitrous ammoniac) is founded on its still retaining this very property. The reader, however, has seen how very faintly it exhibited this criterion of its presence, in the two preceding experiments.

With respect to Sig. Landriani's experiments here referred to, and in which he mixed his supposed different species of fixed air with alkaline air, in the *dry way*, or in phials standing in mercury; I shall only observe that I do not readily conceive how, by a process of this kind, he procured a *sufficient quantity* of *nitrous* ammoniac, so as to ascertain its nature, by detonation; or of *vitriolic* ammoniac, so as to *analyse* it, and know it to be such. All that he says relative to the last-mentioned process, in the
pages

pages now before me, I have given below *. I have more than once indeed *seen* these different *airs* condensed on the sides of a phial, but have been contented with amusing myself by viewing the various configurations of the crystals with a small magnifier, as I despaired of being able to collect them in a sufficient quantity, to subject them to a chemical analysis. Nor have I thought it necessary to prosecute this particular mode of enquiring into the true nature of the acid in fixed air, even since I read these pages of Sig. Landriani, and those sheets of the present work, from which I find that Sig. Fontana, and other philosophers, maintain a doctrine contrary to that which has been advanced in these papers. On some of my former experiments, and on one of them in particular, I think I may safely rest the merits of my present hypothesis.

Your readers will recollect that, in my Vth Experiment, a pure and *acid* fixed air was expelled, *merely by means of heat*, from pure Magnesia, contained in a *glass vessel hermetically* connected with a bent tube. As that Experiment, however, was left somewhat imperfect, by the accidental rupture of my apparatus, which I could not repair or renew; I thought it of sufficient importance to require a careful repetition on the present occasion. I have accordingly more than once repeated it with the utmost attention; and as

* After observing that long and slender crystals were formed on the sides of the phial in which the alkaline and *vitriolic fixed air* were mixed, he only adds, ‘ *Ciò fatto raccolgansi questi cristalli, e se sottopongano all’ analisi, che si troveranno essere un vero sale ammoniaco vitriolico, a cui tante maravigliose virtù attribuisce il chimico Glauber.*’ *Ricerche Fisiche*, &c. p. 48.

the simple apparatus which I employed for this purpose may easily be procured, and the process be as easily repeated by any one, I shall minutely describe the apparatus, and relate all the material circumstances attending the experiment.

EXPERIMENT XX.

I took a very long, small, and thick green glass phial, such as Hungary-water is sometimes sold in, and adapted to it a perforated cork, through which passed a bent tube of a small bore. I filled this phial with the purest Magnesia, pressing it down, that it might harbour as little common air as possible, in the interstices between its particles. Having secured the cork with stiff cement, I put the phial into a crucible, where it was surrounded with sand to the height only of 2 or 3 inches, and which was placed in a small chafing-dish containing lighted charcoal. In consequence of the tallness of the phial, the apparatus remained air-tight, to the end of the process; as the cork and the cement were not affected by the heat, even when the lower part of the phial was red-hot.

Suffering the air to escape while the sand, crucible, &c. were heating, I did not begin to collect any till I judged that the Magnesia was pretty hot. I continued the process till it ceased to emit air. For various purposes I collected the produce in different phials containing water or other liquors. The results were as follow :

1. Though that part of the Magnesia, which was subjected to a moderate red heat, did not, as I afterwards calculated, originally weigh above 1 drachm
and

and a half; I estimated that it furnished above 30 ounces of fixed air.

2. *All the fixed air contained in the Magnesia which occupied the lowest part of the phial, was found to have been expelled from it: at least, on putting some of it afterwards into water, and adding spirit of vitriol to it, it was dissolved in the acid liquor without the least appearance of effervescence.*

3. *The whole of this fixed air (except the first 3 or 4 ounces which were necessarily mixed with some common air) possessed all the qualities of the purest fixed air procured by means of the strongest acid spirits. No difference could be perceived in its properties; except that, probably for the reason just mentioned, it seemed to come over more and more pure as the process went on; and the very last ounce expelled from it (and which appears, from (2) to have been the very last ounce that it contained) was as acid, and was as readily and fully absorbed by water, as any of those that preceded it.—*
To be more particular:

4. During different periods of the process, certain portions of this fixed air neutralised as great a quantity of salt of tartar, as could be neutralised by equal quantities of the purest fixed air obtained by means of the strongest acid spirits.

5. Towards the latter end of the process, two eight-ounce phials filled with rain-water were successively agitated with fresh portions of this fixed air, till they were nearly saturated. Filling up the spaces occupied by the *residua* with water, and throwing up more fixed air, I placed them, inverted, in a basin of mercury. After standing a night, during which they each absorbed an additional ounce or two of fixed air,

their contents were poured out.—I do not remember having ever tasted any artificial Pyrmont-water, made with the vitriolic acid, more brisk and acidulous than this, produced by plain Magnesia, without the intervention of any acid.

As I cannot avoid laying particular stress on the results of this simple calcination of Magnesia; I think it worth while to trace, from it's origin, the fixed air contained in this substance; by explaining the *rationale* of the process by which Magnesia is procured, according to the principles assumed in these papers.

The *Epsom Salt*, from which Magnesia is usually procured, is a neutral compound, formed of the earth of Magnesia combined with the *vitriolic acid*. From this substance the Magnesia is precipitated, by adding to it a solution of salt of tartar; that is, (according to the preceding theory) of an alkaline salt combined with the *mephitic acid*. On mixing together the solutions of these two compounds, the two different acids change bases. The *vitriolic acid* deserts the magnesia, to unite with the alkali, with which it forms a vitriolated tartar; at the same time expelling from the alkali the weaker *mephitic acid*, which instantly occupies its place, by uniting with the magnesia, now deserted by the vitriolic acid.—So that, in fact, all the fixed air, or *mephitic acid*, which, in the preceding experiment, was expelled from the magnesia by fire, had originally resided in the *salt of tartar* employed in the preparation of the magnesia: but as fixed air cannot be expelled from this salt by *heat*, it was therefore, in this Experiment, transferred from it to another body (the earth of magnesia) from which it could be *thus* expelled with ease. The Experi-

periment, in fact, is as satisfactory as if the fixed air had been immediately and directly expelled from the alkaline salt itself by fire.

It may be proper to observe that, in the preparation of pure magnesia, the vitriolated tartar above-mentioned is carefully washed away by repeated ablutions with hot water. But granting, for argument's sake, that the magnesia were not perfectly free from this vitriolic salt, or that some portion of vitriolic acid, supposed still to adhere to it, were capable of being volatilised by the fire, and of being suspended in the fixed air;—neither of which suppositions are, however, admissible;—it is impossible from hence to account for the *large quantity* of acid procured from the magnesia in the preceding experiment, when the reader recollects that 30 ounces of *acid mephitic vapour* were expelled from it, and calculates, from the rough estimate given in my 11th Experiment, the quantity of salt of tartar that it would neutralise. It would indeed be absurd to suppose that this large quantity of acid could be furnished either by any vitriolated tartar, or by any superfluous and disengaged vitriolic acid, still adhering to the magnesia. The sum of the matter is, that acid air resides in mild alkalis, from which it cannot be *directly* expelled, but by other and stronger acids; to which acids, however, it does not owe its acidity, as Signiors Landriani and Fontana affirm, though it may accidentally be adulterated with them.

This experiment appeared to me so decisive of the question in dispute (to say nothing of some others contained in my former letters, particularly those in which *acid* fixed air is expelled, *by heat*, from volatile alkalis)

alkalis) that I could not think it necessary to repeat many similar processes with chalk, or other calcareous earths. It happens fortunately that *all* the fixed air in magnesia can be expelled from it by fire, even in *close* vessels (at least not having a free communication with the atmosphere) and with a moderate degree of heat; whereas I have frequently found, as you have likewise observed, p. 119, that chalk will not *generally*, (for there is a great difference in different specimens of this substance) part with much of its fixed air, under similar, or even more favourable circumstances. And further, the greatest part of the fixed air thus expelled from it, is frequently combined with phlogistic or other matters, which disguise it, and render only a small part of it soluble in water.

I find nevertheless, from your present work, that the foreign philosophers lay much stress on the circumstance, '*that the air expelled from chalk, in close vessels, will not render water acidulous.*' I have always however found, that a sufficient quantity might be expelled from it to redden the infusion of litmus, and sometimes to give a sensibly acidulous impregnation to a small quantity of water. The fact is, that calcareous earths cannot be *calcined*, as magnesia may, in close vessels. Since I perused your sheets, and the above-mentioned pages of Sig. Landriani's treatise, I made the following Experiment relative to this object.

EXPERIMENT XXI.

After having expelled a few ounces of air from 2 or 3 drachms of chalk, exposed to a moderate red heat,

heat, in a glass *vessel*, as in the preceding Experiment ; and having kept the phial in this sand-heat till it would furnish no more, I found that a part only of this air possessed the properties peculiar to fixed air. But I soon discovered the cause of these appearances. On examining the chalk after it was cold, I not only perceived, as you too have observed, that it still effervesced most violently with acids, but, which is a much more decisive circumstance, I found that *nearly the whole of its fixed air still remained in it* ; for on throwing the chalk into two ounces of water, I observed that it had not acquired the property of communicating, even to this small portion of water, the taste of *lime-water* ; nor, after standing a day or two, was any perceptible crust formed upon its surface. And yet, from Dr. Black's well-known experiments, it is clear, that if even a *single grain* of this chalk had been calcined, or deprived of *all* its fixed air, it must have converted the 2 ounces of water into a pretty strong lime-water. In short, it evidently appeared, that the mephitic acid which, when expelled, should render the water acidulous, still remained in the chalk at the end of the experiment ; and that the greater part of the air that did come over, was either not fixed air, or was fixed air enveloped in phlogistic matters, or otherwise so altered, as not to exhibit its usual properties ; whereas *acids* expel fixed air from calcareous substances wholly, and in a state of purity, equal to that in which it is expelled, by *fire*, from magnesia ; though even in this last case, it suffers a remarkable change, when the magnesia is calcined in a gun-barrel.

Before

Before I conclude this enquiry, I shall take notice of a curious and difficult problem in chemistry, relative to fixed air, and which no one, I believe, has yet attempted to resolve. In the decomposition both of *common* and *cubic* nitre, by deflagration with charcoal in a red-hot crucible, it is very remarkable that the alkaline basis of the nitre is in both cases left, not in a *caustic* state, as might be expected, but replete with fixed air, or in a *mild* state. It seems, at first sight, pretty evident that the alkaline salt acquires this large quantity of fixed air at the instant of the deflagration †; and as there are only two substances present from which it can acquire it, the nitrous acid and the charcoal, it may be conjectured — (and indeed some of your experiments contained in this volume seem to favour the idea) — that some part of the nitrous acid, which, as to sense, appears to be totally dissipated in the process, may assume the modification of fixed air, and be instantly condensed and combined with the alkali under that form.

This solution, admitting it to be just, overturns the hypothesis of Sig. Landriani, and the foreign philosophers above mentioned: for, supposing fixed air to be afterwards procured from this alkali, by means of oil of vitriol, marine acid, the acid of lemons, or, in short, any other acid than the *nitrous*; their theory would oblige them to ascribe it's acidity to the particular adventitious acid employed in the expulsion of

† Unless it should be supposed that it attracts it afterwards from the atmosphere, during the subsequent part of the process; when it is generally kept in a red heat for half an hour after the deflagration is over: — a circumstance which I have not enquired into, but which might be ascertained by examining it immediately after the deflagration.

it : whereas, according to this solution, the acid in the fixed air must, in all these cases, be the nitrous. I must not however omit to observe that it appears, from one of the sheets of the present volume now before me, (page 214.) that charcoal contains a considerable quantity of fixed air ; from which possibly the alkali, deserted by the nitrous acid during the deflagration, may obtain that principle.—The question certainly deserves to be further inquired into ; as a just solution of it promises to throw considerable light on the intimate nature, or *genesis* of fixed air.

On the whole, I think it will appear evident from the preceding Experiments, and particularly the XXth, that the acid contained in fixed air, procured in the usual method, by means of oil of vitriol, and other acids, is not, as Sig. Landriani and others assert, merely the attenuated and dissolved vapour of the foreign acid employed in the process ; but that it is a distinct principle, expelled, in an acid state, from the body to which these stronger acids had been applied. The mephitic acid has, in all my Experiments, appeared (impurities excepted) to be an invariable, homogeneous substance, which does not exhibit any of those varieties which might certainly be expected in it, if it owed its existence to acids differing so very considerably from each other in their properties, as those usually employed in the procuring it. It is not my present design to deduce the generation of the mephitic acid, *ab ovo usque*—. It is sufficient to observe that it appears from its qualities to be as distinct from the vitriolic, nitrous, and other known acids, as they are from each other. They may all, as Becher and Stahl long ago supposed, be only modifica-

difications of one and the same primitive and universal acid. But this leads to an enquiry utterly foreign the object I proposed to myself in prosecuting these experiments.

Great Massingham,
Nov. 27, 1775.

I am, &c.

W. BEWLY,

P. S. I willingly lay hold of this opportunity of following your example, in rectifying a mistake of Sig. Landriani's respecting myself, which occurs at page 23 of his treatise above referred to; into which he has probably been led, either through his imperfect knowledge of our language, or the mistake of a translator. He there represents me as maintaining 'that nitrous air is nothing more than *common air*, containing the nitrous acid dissolved in it, combined with phlogiston.'—On the contrary, I concluded, from my experiments related in your former volume, page 317, that nitrous air consisted of the nitrous acid combined with phlogiston; and so far from considering common air as a *component* principle, or the *basis*, of nitrous air, I shewed that an addition of common air was necessary, in order to *decompound* and condense it.

I seize likewise this opportunity of recommending to the consideration and trial of the faculty, the new neutral salt indicated in my VIIIth Experiment; both as it is a new and untried saline compound, and as much benefit may be expected from it, even *a priori*, from the known properties and activity of fixed air, largely introduced into the system; particularly as a febrifuge and antiseptic, in fevers, and other disorders,

ders, of a putrid tendency.—As to the preparation of it—though I had before found that in proportion as the alkaline salt approaches to the state of neutralisation, it attracts the mephitic acid more weakly; yet since I wrote the preceding letter, I have prepared near 3 pints of a neutral solution of this kind, in Dr. Nooth's apparatus, as improved by Mr. Parker, which contained 10 grains of salt of tartar in each ounce of water. Notwithstanding the unavoidable dissipation of the fixed air in the upper vessel, the alkaline solution was rendered perfectly neutral in about 24 hours, in consequence of frequent agitation, and the successive addition of fresh portions of fixed air; and, after suffering it to stand two or three days longer, it became pleasant to the taste, strongly acidulous, and even pungent. My acquaintance with this neutral julep is of too late a date to enable me to add any thing material to what I have before said of it. The present, indeed, is not a fit season to quaff large potations of cold water, by way of experiment.

Nov. 29, 1775.

E R R A-

E R R A T A.

PAGE 27, line 12 read, *was considerably injured by it* ; leaving out the Remainder of the Paragraph, in which there is some Error, that I cannot rectify without repeating the Experiment.

P. 41, l. 8, for *redress* r. *redness*.

P. 49, l. 4, for *common* r. *fixed*.

P. 88, l. 21, for *air*, r. *air* ;

P. 315, l. 21, for *injured air* r. *injured*.

THE

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